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XXXI. *Experimental Researches in Electricity.—Third Series.*

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- § 7. *Identity of Electricities derived from different Sources.*
§ 8. *Relation by Measure of common and Voltaic Electricity.*

§ 7. *Identity of Electricities derived from different Sources.*

265. **T**HE progress of the electrical researches which I have had the honour to present to the Royal Society, brought me to a point at which it was essential for the further prosecution of my inquiries that no doubt should remain of the identity or distinction of electricities excited by different means. It is perfectly true that Cavendish†, Wollaston‡, Coladon§ and others, have in turn removed some of the greatest objections to the acknowledgement of the identity of common, animal and voltaic electricity, and I believe that philosophers generally consider these electricities as really the same. But on the other hand it is also true, that the accuracy of Wollaston's experiments has been denied||, and that one of them, which really is no proof of chemical decomposition by com-

* From the Phil. Trans. for 1833, Part I. This paper was read before the Royal Society, Jan. 10 and 17, in the present year. Abstracts of the author's First and Second Series of Experimental Researches in Electricity, containing § 1 to § 6, will be found in Phil. Mag. and Annals, N.S. vol. xi. pp. 445, 447.

† Phil. Trans. 1776, p. 196.

‡ Ibid. 1801, p. 434.

§ *Annales de Chimie*, 1826, p. 62, &c.

|| Phil. Trans. 1832, p. 282, note.

mon electricity (309. 327.), has been that selected by several experimenters as the test of chemical action (336. 346.). It is a fact, too, that many philosophers are still drawing distinctions between the electricities from different sources; or at least doubting whether their identity is proved. Sir Humphry Davy, for instance, in his paper on the Torpedo*, thought it probable that animal electricity would be found of a peculiar kind; and referring to that, in association with common electricity, voltaic electricity and magnetism, has said, "Distinctions might be established in pursuing the various modifications or properties of electricity in these different forms, &c." Indeed I need only refer to the last volume of the Philosophical Transactions to show that the question is by no means considered as settled†.

266. Notwithstanding, therefore, the general impression of the identity of electricities, it is evident that the proofs have not been sufficiently clear and distinct to obtain approbation from all those who were competent to consider the subject; and the question seemed to me very much in the condition of that which Sir H. Davy solved so beautifully,—namely,

* Phil. Trans. 1829, p. 17. "Common electricity is excited upon non-conductors, and is readily carried off by conductors and imperfect conductors. Voltaic electricity is excited upon combinations of perfect and imperfect conductors, and is only transmitted by perfect conductors or imperfect conductors of the best kind. Magnetism, if it be a form of electricity, belongs only to perfect conductors; and, in its modifications, to a peculiar class of them^a. Animal electricity resides only in the imperfect conductors forming the organs of living animals, &c."

† Phil. Trans. 1832, p. 259. Dr. Davy, in making experiments on the torpedo, obtains effects the same as those produced by common and voltaic electricity, and says that in its magnetic and chemical power it does not seem to be essentially peculiar,—p. 274; but he then says, p. 275, there are other points of difference; and after referring to them, adds, "How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. Cavendish in his theory of the torpedo; or may we suppose, according to the analogy of the solar ray, that the electrical power, whether excited by the common machine, or by the voltaic battery, or by the torpedo, is not a simple power, but a combination of powers, which may occur variously associated, and produce all the varieties of electricity with which we are acquainted?"

At p. 279 of the same volume of Transactions is Dr. Ritchie's paper, from which the following are extracts: "Common electricity is diffused over the surface of the metal;—voltaic electricity exists within the metal. Free electricity is conducted over the surface of the thinnest gold leaf as effectually as over a mass of metal having the same surface;—voltaic electricity requires thickness of metal for its conduction," p. 280: and again, "The supposed analogy between common and voltaic electricity, which was so eagerly traced after the invention of the pile, completely fails in this case, which was thought to afford the most striking resemblance," p. 291.

^a Dr. Ritchie has shown this is not the case, Phil. Trans. 1832, p. 294.

whether voltaic electricity in all cases merely eliminated, or did not in some actually produce, the acid and alkali found after its action upon water. The same necessity that urged him to decide the doubtful point, which interfered with the extension of his views, and destroyed the strictness of his reasoning, has obliged me to ascertain the identity or difference of common and voltaic electricity. I have satisfied myself that they are identical, and I hope the proofs I have to offer, and the results flowing from them, will be found worthy the attention of the Royal Society.

267. The various phænomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads; namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient. The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances. The effects of electrical currents may be considered as 1st, Evolution of heat; 2nd, Magnetism; 3rd, Chemical decomposition; 4th, Physiological phænomena; 5th, Spark. It will be my object to compare electricities from different sources, and especially common and voltaic electricities, by their power of producing these effects.

I. Voltaic Electricity.

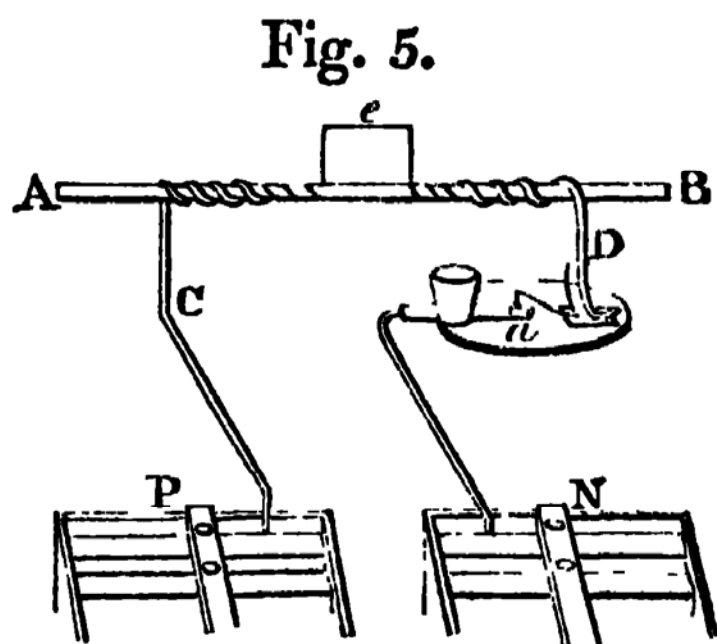
268. *Tension.*—When a voltaic battery of 100 pairs of plates has its extremities examined by the ordinary electrometer, it is well known that they are found positive and negative, the gold leaves at the same extremity repelling each other, the gold leaves at different extremities attracting each other, even when half an inch or more of air intervenes.

269. That ordinary electricity is discharged by points with facility through air; that it is readily transmitted through highly rarefied air; and also through heated air, as for instance a flame; is due to its high tension. I sought, therefore, for similar effects in the discharge of voltaic electricity, using as a test of the passage of the electricity either the galvanometer or chemical action produced by the arrangement hereafter to be described (312. 316.).

270. The voltaic battery I had at my disposal consisted of 140 pairs of plates four inches square, with double coppers. It was insulated throughout, and diverged a gold leaf electrometer about one third of an inch. On endeavouring to discharge this battery by delicate points very nicely arranged and approximated, either in the air or in an exhausted receiver, I could obtain no indications of a current, either by

magnetic or chemical action. In this, however, was found no point of discordance between voltaic and common electricity; for when a Leyden battery (291.) was charged so as to deflect the gold leaf electrometer equally, the points were found equally unable to discharge it with such effect as to produce either magnetic or chemical action. This was not because common electricity could not produce both these effects (307. 310.), but because when of such low intensity the quantity required to make the effects visible (being enormously great (371. 375.)) could not be transmitted in any reasonable time. In conjunction with the proofs of identity hereafter to be given, these effects of points also prove identity instead of difference between voltaic and common electricity.

271. As heated air discharges common electricity with far greater facility than points, I hoped that voltaic electricity might in this way also be discharged. An apparatus was therefore constructed (fig. 5.), in which AB is an insulated glass rod upon which two copper wires, C, D, are fixed firmly; to these copper wires are soldered two pieces of fine platina wire, the ends of which are brought very close to each other at *e*, but without touching; the copper wire C was connected with the positive pole of a voltaic battery, and the wire D with a decomposing apparatus (312. 316.), from which the communication was completed to the negative pole of the battery. In these experiments only two troughs, or twenty pairs of plates, were used.



272. Whilst in the state described, no decomposition took place at the point *a*, but when the side of a spirit-lamp flame was applied to the two platina extremities at *e*, so as to make them bright red-hot, decomposition occurred; iodine soon appeared at the point *a*, and the transference of electricity through the heated air was established. On raising the temperature of the points *e* by a blowpipe, the discharge was rendered still more free, and decomposition took place instantly. On removing the source of heat, the current immediately ceased. On putting the ends of the wires very close by the side of and parallel to each other, but not touching, the effects were perhaps more readily obtained than before. On using a larger voltaic battery (270.), they were also more freely obtained.

273. On removing the decomposing apparatus and inter-

posing a galvanometer instead, heating the points *e* as the needle would swing one way, and removing the heat during the time of its return (302.), feeble deflections were soon obtained: thus also proving the current through heated air; but the instrument used was not so sensible under the circumstances as chemical action.

274. These effects, not hitherto known or expected under this form, are only cases of the discharge which takes place through air between the charcoal terminations of the poles of a powerful battery, when they are gradually separated after contact. Here the passage is through heated air exactly as with common electricity, and Sir H. Davy has recorded that with the original battery of the Royal Institution this discharge passed through at least four inches of air*. In the exhausted receiver the electricity would *strike* through nearly half an inch of space, and the combined effects of rarefaction and heat was such upon the inclosed air as to enable it to conduct the electricity through a space of six or seven inches.

275. The instantaneous charge of a Leyden battery by the poles of a voltaic apparatus is another proof of the tension, and also the quantity, of electricity evolved by the latter. Sir H. Davy says†, “When the two conductors from the ends of the combination were connected with a Leyden battery, one with the internal, the other with the external coating, the battery instantly became charged, and on removing the wires and making the proper connexions, either a shock or a spark could be perceived: and the least possible time of contact was sufficient to renew the charge to its full intensity.”

276. *In motion. i. Evolution of Heat.*—The evolution of heat in wires and fluids by the voltaic current is matter of general notoriety.

277. *ii. Magnetism.*—No fact is better known to philosophers than the power of the voltaic current to deflect the magnetic needle, and to make magnets according to *certain laws*; and no effect can be more distinctive of an electrical current.

278. *iii. Chemical decomposition.*—The chemical powers of the voltaic current, and their subjection to *certain laws*, are also perfectly well known.

279. *iv. Physiological Effects.*—The power of the voltaic current, when strong, to shock and convulse the whole animal system, and when weak to affect the tongue and the eyes, is very characteristic.

280. *v. Spark.*—The brilliant star of light produced by the

* Elements of Chemical Philosophy, p. 153.

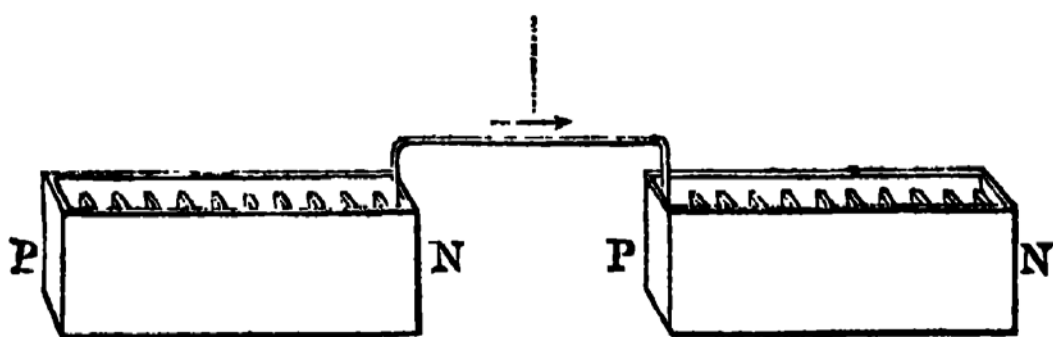
† Ibid. p. 154.

discharge of a voltaic battery is known to all as the most beautiful light that man can produce by art.

281. That these effects may be almost infinitely varied, some being exalted whilst others are diminished, is universally acknowledged; and yet without any doubt of the identity of character of the voltaic currents thus made to differ in their effect. The beautiful explication of these variations afforded by Cavendish's theory of quantity and intensity requires no support at present, as it is not understood to be doubted.

282. In consequence of the comparisons that will hereafter arise between wires carrying voltaic and ordinary electricities, and also because of certain views of the condition of a wire or any other conducting substance connecting the poles of a voltaic apparatus, it will be necessary to give some definite view of what is called the voltaic current, in contradistinction to any supposed peculiar state of arrangement, not progressive, which the wire or the electricity within it may be supposed to assume. If two voltaic troughs PN, P'N', fig. 1. be symmetri-

Fig. 1.



cally arranged and insulated, and the ends NP' connected by a wire, over which a magnetic needle is suspended, the wire will exert no effect over the needle; but immediately that the ends PN' are connected by another wire, the needle will be deflected, and will remain so as long as the circuit is complete. Now if the troughs merely act by causing a peculiar arrangement in the wire either of its particles or its electricity, that arrangement constituting its electrical and magnetic state, then the wire NP' should be in a similar state of arrangement *before* P and N' were connected, to what it is afterwards, and should have deflected the needle, although less powerfully, perhaps to one half the extent which would result when the communication is complete throughout. But if the magnetic effects depend upon a current, then it is evident why they could not be produced in *any* degree before the circuit was complete; because prior to that no current could exist.

283. By *current*, I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or, speaking still more ge-

nerally, progressive forces. By *arrangement*, I understand a local adjustment of particles, or fluids, or forces, not progressive. Many other reasons might be urged in support of the view of a *current* rather than an *arrangement*, but I am anxious to avoid dilating unnecessarily upon what can be supplied by others at the moment.

II. *Ordinary Electricity.*

284. By ordinary electricity I understand that which can be obtained from the common machine, or from the atmosphere, or by pressure, or cleavage of crystals, or by a multitude of other operations; its distinctive character being that of great intensity, and the exertion of attractive and repulsive powers, not merely at sensible but at considerable distances.

285. *Tension.* The attractions and repulsions at sensible distances, caused by ordinary electricity, are well known to be so powerful in certain cases, as to surpass, almost infinitely, the similar phænomena produced by electricity, considered as of other kinds. But still those attractions and repulsions are exactly of the same nature as those already referred to under the head *Tension, Voltaic electricity* (268.); and the difference in degree between them is not greater than often occurs between cases of ordinary electricity only. I think it will be unnecessary to enter minutely into the proofs of the identity of this character in the two instances. They are abundant; are generally admitted as good; and lie upon the surface of the subject: and whenever in other parts of the comparison I am about to draw, a similar case occurs, I shall content myself with a mere announcement of the similarity, expanding only upon those parts where the great question of distinction or identity is still controverted.

286. The discharge of common electricity through heated air is a well-known fact. The parallel case of voltaic electricity has already been described (272, &c.).

287. *In motion. i. Evolution of Heat.* The heating power of common electricity when passed through wires or other substances, is perfectly well known. The accordance between it and voltaic electricity is in this respect complete. Mr. Harris has constructed and described* a very beautiful and sensible instrument on this principle, in which the heat produced in a wire by the discharge of a mere spark of common electricity is readily shown, and to which I shall have occa-

* Philosophical Transactions, 1827, p. 18. Edinburgh Transactions, 1831. Harris on a New Electrometer, &c. &c.

sion to refer for experimental proof in a future part of this paper (344.).

288. ii. *Magnetism*. Voltaic electricity has most extraordinary and exalted magnetic powers. If common electricity be identical with it, it ought to have the same powers. In rendering needles or bars magnetic, it is found to agree with voltaic electricity, and the *direction* of the magnetism, in both cases, is the same; but in deflecting the magnetic needle, it has been found deficient, so that sometimes the power has been denied altogether, and at other times distinctions have been hypothetically assumed for the purpose of avoiding the difficulty*.

289. M. Colladon, of Geneva, considered that the difference might be due to the use of insufficient quantities of common electricity in all the experiments before made upon this point; and in a memoir read to the *Académie des Sciences* in 1826†, describes experiments, in which, by the use of a battery, points, and a delicate galvanometer, he succeeded in obtaining deflections, and thus establishing identity in that respect. MM. Arago, Ampere, and Savary are mentioned in the paper as having witnessed a successful repetition of the experiments. But as no second witness of these effects has come forward, MM. Arago, Ampere, and Savary, not having themselves published (that I am aware of,) their acceptance of the results, and as others have not been able to obtain the effects, M. Colladon's conclusions have been by some doubted or denied; and an important point with me was to establish their accuracy, or remove them entirely from the received body of experimental evidence. I am happy to say that my results fully confirm those by M. Colladon, and I should have had no occasion to describe them, but that they are essential as proofs of the accuracy of the final and general conclusions I am enabled to draw respecting the magnetic and chemical action of electricity, (360. 366. 367. 377. &c.).

290. The plate electrical machine I have used is fifty inches in diameter; it has two sets of rubbers; its prime conductor consists of two brass cylinders connected by a third, the whole length being twelve feet, and the surface in contact with air about 1422 square inches. When in good excitation, one revolution of the plate will give ten or twelve sparks from the conductors, each an inch in length. Sparks or flashes from ten to fourteen inches in length may easily be drawn from the

* Demonferrand's *Manuel d'Electricité dynamique*, p. 121.

† *Annales de Chimie*, xxxiii. p. 62.

conductors. Each turn of the machine, when worked moderately, occupies about $\frac{4}{3}$ ths of a second.

291. The electric battery consisted of fifteen equal jars. They are coated eight inches upwards from the bottom, and are twenty-three inches in circumference, so that each contains one hundred and eighty-four square inches of glass, coated on both sides, independent of the bottoms, which are thicker glass, and contain each about fifty square inches.

292. A good discharging train was arranged by connecting metallically a sufficiently thick wire with, first, the metallic gas pipes of the house, then with the metallic gas pipes belonging to the public gas works of London; and finally, with the metallic water pipes of London. It was so effectual as to carry off instantaneously electricity of the feeblest tension, even that of a single voltaic trough, and was essential to many of the experiments.

293. The galvanometer was one or the other of those formerly described (87. 205.), but the glass jar covering it and supporting the needle was coated inside and outside with tin-foil, and the upper part (left uncoated, that the motions of the needle might be examined,) was covered with a frame of wire-work, having numerous sharp points projecting from it. When this frame and the two coatings were connected with the discharging train (292.), an insulated point or ball, connected with the machine when most active, might be brought within an inch of any part of the galvanometer, yet without affecting the needle within by any ordinary electrical attraction or repulsion.

294. In connexion with these precautions, it may be necessary to state that the needle of the galvanometer is very liable to have its magnetic power deranged, diminished, or even inverted by the passage of a shock through the instrument. If the needle be at all oblique in the wrong direction to the coils of the galvanometer, when the shock passes, effects of this kind are sure to happen.

295. It was to the retarding power of bad conductors, that I first looked with the hope of being able to make common electricity assume more of the characters and power of voltaic electricity, than it is usually supposed to have.

296. The coating and armour of the galvanometer were first connected with the discharging train (292.); the end B (87.) of the galvanometer wire was connected with the outside coating of the battery, and then both these with the discharging train; the end A of the galvanometer wire was connected with a discharging rod by a wet thread four feet long;

and finally, when the battery had been positively charged by about forty turns of the machine, it was discharged by the rod and the thread through the galvanometer. The needle immediately moved.

297. During the time that the needle completed its vibration in the first direction and returned, the machine was worked, and the battery charged; and when the needle in vibrating resumed its first direction, the discharge was again made through the galvanometer. By repeating this action a few times, the vibrations soon extended to above 40° on each side of the line of quiescence.

298. This effect could be obtained at pleasure. Nor was it varied, apparently, either in direction or degree, by using a short thick string, or even four short thick strings in place of the long fine thread. With a more delicate galvanometer, an excellent swing of the needle could be obtained by one discharge of the battery.

299. On reversing the galvanometer communications so as to pass the discharge through from B to A, the needle was equally well deflected, but in the opposite direction.

300. The deflections were in the same direction as if a voltaic current had been passed through the galvanometer, i. e. the positively charged surface of the electric battery coincided with the positive end of the voltaic apparatus (268.), and the negative surface of the first with the negative end of the latter.

301. The battery was then thrown out of use, and the communications so arranged that the current could be passed from the prime conductor, by the discharging rod held against it, through the wet string, through the galvanometer coil, and into the discharging train (292.), by which it was finally dispersed. This current could be stopped at any moment, by removing the discharging rod, and either stopping the machine or connecting the prime conductor by another rod with the discharging train; and could be as instantly renewed. The needle was so adjusted, that whilst vibrating in moderate and small arcs, it required time equal to twenty-five beats of a watch to pass in one direction through the arc, and of course an equal time to pass in the other direction.

302. Thus arranged, and the needle being stationary, the current, direct from the machine, was sent through the galvanometer for twenty-five beats, then interrupted for other twenty-five beats, renewed for twenty-five beats more, again interrupted for an equal time, and so on continually. The needle soon began to vibrate visibly, and after several

alternations of this kind, the vibration increased to 40° or more.

303. On changing the direction of the current through the galvanometer, the direction of the deflection of the needle was also changed. In all cases the motion of the needle was the same in direction as that caused by the use of an electric battery or a voltaic trough (300.).

304. I now rejected the wet string, and substituted a copper wire, so that the electricity of the machine passed at once into wires communicating directly with the discharging train, the galvanometer coil being one of the wires used for the discharge. The effects were exactly those obtained above (302.).

305. Instead of passing the electricity through the system, by bringing the discharging rod at the end of it into contact with the conductor, four points were fixed on to the rod; when the current was to pass, they were held about twelve inches from the conductor, and when it was not to pass, they were turned away. Then, except with this variation operating as before (302.), the needle was soon powerfully deflected, and in perfect consistency with the former results. Points afforded the means by which Colladon, in all cases, made his discharges.

306. Finally, I passed the electricity first through an exhausted receiver, so as to make it there resemble the aurora borealis, and then through the galvanometer to the earth; and it was found still effective in deflecting the needle, and apparently with the same force as before.

307. From all these experiments, it appears that a current of common electricity, whether transmitted through water or wire, or rarefied air, or by means of points in common air, is still able to deflect the needle; the only requisite being, apparently, to allow time for its action: that it is, in fact, just as magnetic in every respect as a voltaic current, and that in this character therefore no distinction exists.

308. Imperfect conductors, as water, brine, acids, &c. &c. will be found far more convenient for exhibiting these effects than other modes of discharge, as by points or balls; for the former convert at once the charge of a powerful battery into a feeble spark, or rather continuous current, and involve little or no risk of deranging the magnetism of the needles (294.).

[To be continued.]

XXXII. *On the Inflection of Light, in Reply to Professor Powell.* By JOHN BARTON, Esq.*

I AM much indebted to Professor Powell for the candid and friendly manner in which he has commented on the objections to the undulatory theory of light, which I adduced in my former communication. On the present occasion I propose, first, to make a few remarks in explanation or justification of what I before advanced; and then to suggest another hypothesis, which I persuade myself is capable of explaining the phænomena of inflection more completely and satisfactorily than that of Fresnel.

The first question at issue between the undulationists and myself relates to a matter of fact. Newton found that when a beam of light is suffered to fall on a very narrow slit, the beam parts in the middle, leaving a dark space between the two portions into which it is divided. This result agrees with my own observation. Professor Airy, on the contrary, finds the centre of the spectrum always relatively bright, however narrow the slit through which the light passes. I have no wish to disparage Professor Airy's authority; but I must be permitted to prefer the testimony of my own senses, supported as it is by the observations of Newton, and even, I think I may add, by the observations of Professor Powell. For although this gentleman has not succeeded in reproducing the phenomena under the exact form described by me, and delineated in my last communication, he has said enough to convince every one who reflects on the subject, of the possibility of so succeeding when the experiment is repeated with due precaution. "When," says he, "the edges approached very nearly, I observed something like the appearance described by Mr. Barton, but with this difference, that the boundaries of the dark space, instead of being continuous, diverged either way into the shadow in hyperbolic lines." Now, I am sure that if Professor Powell will reconsider the question, he will see that, from the nature of the hyperbola, there must, on his own showing, be a certain width of interval between the two knife-blades, giving a result such as I have described. In fact, it is easy, from the hyperbolic form of the lines, combined with Newton's observation that the dark space begins to appear when the interval between the two blades is less than the four-hundredth part of an inch, to assign the precise distance at which the blades must be placed, and the degree of curvature they must possess, in order to give a dark space of any required figure and dimensions. I need not dwell on this point,

* Communicated by the Author: see our preceding volume, p. 424.

since the details of the computation will be readily supplied by any one versed in mathematics. But I may add, that if Professor Powell will employ blades of less curvature, he will find the experiment succeed more readily. Those employed by him in repeating my experiment had, if I do not mistake, so inconsiderable a radius of curvature as to reduce the dimensions of the dark space within a limit too small to be conveniently observed.

It would seem, indeed, from his subsequent observations, that Professor Powell is not quite sure whether to admit or not the correctness of Newton's observation; for after saying that he cannot succeed in obtaining the results described by me, he goes on to assign certain considerations which may serve, he thinks, to reconcile the phænomena with the undulatory hypothesis, *even supposing* the facts to be such as I have asserted. The formulæ of Fresnel, he says, always suppose the opening through which the light enters the darkened chamber to be a mathematical point: whereas Newton employed for this purpose a hole a quarter of an inch in breadth. But the experiment succeeds with me equally well, whether the opening in question be large or small, while it fails equally in either case with Professor Airy; this cannot, therefore, be the source of the different results obtained by us. Indeed it appears quite incredible that the enlargement or contraction of the aperture by which the light enters the chamber can produce a difference *of such a kind* as supposed by Professor Powell. That a change in the dimensions of the aperture should affect the breadth or intensity of the dark space may be readily imagined; but that such a change should convert light into darkness appears inconceivable.

The second observation of Professor Powell under this head I am apprehensive I do not fully comprehend. He says, "It results from the well-known fact of the enlargement of the shadows of the two edges beyond their geometrical boundaries, that these shadows will coalesce before the edges meet;.....and this seems very likely to have been the real result observed by Newton." Surely the formulæ of Fresnel are intended to comprise the whole of the phenomena, including among the rest that enlargement of the shadow to which Professor Powell alludes. If so, it cannot be allowable to employ this enlargement as an after correction to the results of calculations deduced from those formulæ.

With regard to another objection advanced by me against the theory of Fresnel,—its inconsistency with the observations of Newton and Biot on the distance at which the first of the dark bands cross one another in the centre of the spec-

trum,—Professor Powell thinks these observations not to be relied on, as being of earlier date, and performed with apparatus less susceptible of accuracy than that employed by Fresnel. I must observe, however, that the question at issue does not rest on the comparative accuracy of the methods of observation employed. Fresnel has not recorded any experiments made under circumstances similar to those which gave to Newton and Biot results so much at variance with his theory. He examined in two cases only the distance at which the first dark bands are found to intersect. In these two cases the width of the slit was, respectively, two millimetres, and one millimetre and a half*; whereas the discordant results given by the observations of Newton and Biot relate to widths below one millimetre, or the twenty-fifth part of an inch. Even, therefore, if the observations of Fresnel were as far superior in accuracy to those of his predecessors as Professor Powell assumes, it would not be decisive in favour of his theory. But I confess I cannot see any sufficient ground for attributing to the observations of Fresnel such unmeasured superiority as has been supposed. I cannot think that if Fresnel deserves all confidence to the hundredth part of a millimetre, Newton and Biot are unworthy of being trusted to the tenth part of a millimetre, as Professor Powell's reasoning implies. I cannot think that the accuracy of different series of observations is to be estimated solely by their relative antiquity. Fresnel certainly introduced one important improvement into the method of observation—he received the rays on a lens, instead of receiving them on a sheet of paper, like Newton, or on a piece of roughened glass, like Biot: he was thus enabled to measure the intervals between the fringes by a micrometer,—a method which admits of greater accuracy than the earlier method of taking off the distance by a pair of compasses. But let it be observed that in the particular class of phenomena at present under consideration, the advantage gained by this improvement in the method of observation ceases altogether; since the question relates, not to the width of the interval between two fringes, but to the place of their intersection. With regard to another important point, the measurement of the width of the slit, there is some reason to think Newton's method of observation preferable to Fresnel's. I do not, indeed, find in Fresnel's memoir any account of the manner in which this was accomplished in his experiments; but Newton has described the method employed by himself. Having placed his knife-blades with their edges (which were

* *Mémoires de l'Institut*, tome v. pp. 398 and 437.

ground “truly straight,”) facing each other, and their points meeting at a given small angle, he computed the interval between them at any distance from that angle, by the problem of similar triangles. This method appears susceptible of greater accuracy than any process of direct measurement. If, therefore, Fresnel employed some such direct means, I should be disposed to place greater confidence in the observations of Newton, who was no less remarkable for accuracy as an experimentalist than for genius as a mathematician. Professor Airy, indeed, finds Newton’s measurements bad, because, “according to theory, when the breadth of the aperture is very small, the distance should vary as the square of the breadth,” which is not found to be the case in Newton’s observations. But I would ask, Is not this taking for granted the very thing to be proved? When we are seeking to bring to test the soundness of a theory by comparing it with the results of observation, can it be allowable to measure the accuracy of those very observations by their accordance or discordance with the theory in question?

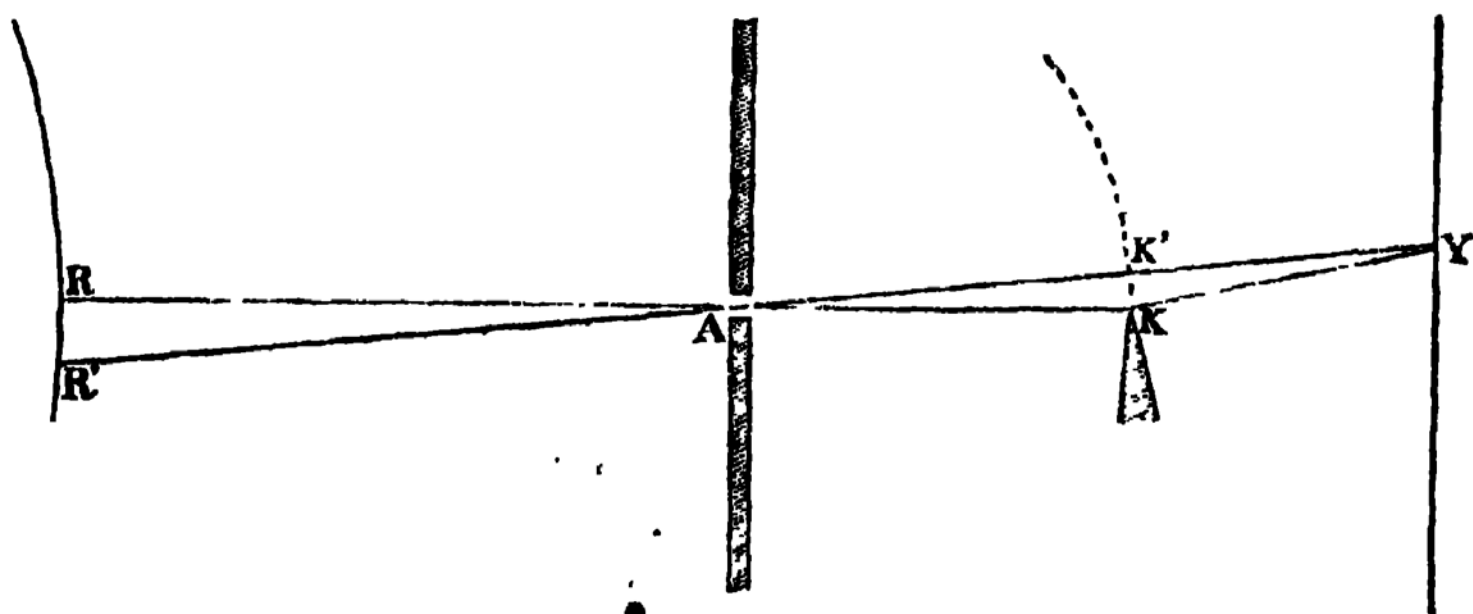
The results obtained by Newton and Biot display, I think, strong internal evidence that their discrepancy with the theory of Fresnel is not attributable solely to errors of observation. It will be observed, on examining those results, that when the width of the slit exceeds the thirtieth part of an inch, or thereabouts, the observations are not greatly at variance with the theory; but in proportion as the width is reduced below this limit, the discordance between theory and observation increases; and at length, when the width is less than the four-hundredth part of an inch, it becomes a difference, not only in degree, but in kind; the theory indicating that the spectrum should be brightest at the centre, while observation shows that this centre is quite dark. Looking at this progressively increasing discrepancy between the conclusions of theory and the observed phænomena, it is difficult to avoid suspecting that it depends on some defect in the theory itself.

In addition to these objections to the theory of Fresnel, founded on its want of agreement with observed phænomena, I suggested, in my former communication*, an objection of another kind. I endeavoured to show that the origin of the undulations supposed to constitute light is placed in this theory at a wrong point. Fresnel and his followers placed this origin at the point of intersection—whether a pin-hole through which the rays enter the darkened chamber, or the focus of a lens occupying the same position. To me it appears that

* See Lond. and Edinb. Phil. Mag. vol. ii. p. 263.—*EDIT.*

the true origin of the undulations should be placed at the sun's surface. I am sure, from the language of Professor Powell's reply, that I did not explain myself on this head so fully and explicitly as I ought. He says, "If the aperture be very small, it is shown by theory that the new wave diverging from it, which is produced by the sum of all the small waves belonging to the original waves from RR' , &c., will be equally strong in all directions; and A in this case is the real centre and origin from which the new undulations commence."

In our sun, it is no doubt true, as observed by Professor Powell, that the wave diverging from the point of intersection "is produced by the sum of all the small waves belonging to the original waves from RR' &c." It is no doubt true, on the principles of the undulatory theory, that the particles of æther composing the wave $R'A$, on arriving at the point A are agitated by a multitude of impulses derived from other points of the sun's surface; the succeeding portion AY may therefore be considered as the joint result of all these impulses. But inasmuch as the disturbing forces in question are infinite in number, and infinitely various in force and direction, they completely counteract each other's operation; and the practical result is, that the wave emanating from the point R' goes forward to its ultimate destination at Y , precisely as if it had not been crossed in its path by any other rays whatever. Such at least was the doctrine of Huygens, the great author of the undulatory theory. If the undulationists of the present day have seen it necessary to abandon or modify the principles of their master, it appears incumbent on them to assign some reason for the change, and to show how it may be reconciled



with the laws which are admitted to regulate the undulations of elastic media.

According to Young, the point Y will always be of double brightness if the paths of the two rays AY , AKY differ in length by an integral number of undulations. But it is evident that the chances are infinite against the two rays RA , $R'A$,

arriving at the point A in the same state: their conditions must be as diversified as the lengths of the paths which they have severally traversed in arriving from the sun. What then is the nature of the force which operates upon them at A, the point of intersection, to bring them, or one of them, into a new condition? Suppose, for instance, that the wave forming the ray RA, on arriving at the point A, happens to be at its maximum of advance, while the wave forming the ray R'A, on arriving at the same point, happens to be at its maximum of retrogression. By the theory of Huygens, each will go forward in its course quite independently, as if the other did not exist. How happens it, then, that on arriving at K and K' they are found in one and the same state? One or both of them must have experienced an acceleration, or a retardation, such as to make up together the length of half an undulation. How has this been accomplished?

In the theory of Young, respect is had only to the two rays RA, R'A, on whose interference the colour of the point Y is supposed to depend. In the theory of Fresnel, the new wave diverging from A is supposed, as Professor Powell truly says, to be produced by the sum of all the small waves belonging to the original waves from R R', &c. But this, though a more accurate way of stating the question than Young's, gives in practice very nearly the same results; inasmuch as those parts only of the wave entering the chamber which follow the direction of the lines AY, AKY, do in fact constitute *effectual rays* ("rayons efficaces"), as Fresnel has himself observed. It is essential to his theory that every part of the hemispherical wave K'K should be found in one and the same condition, as it is to the theory of Young that the two individual rays should be found in like condition at K' and K. The simple question which I would ask of the undulationists is, how has this equality of condition been brought about? I do not see how it is possible; on their theory, to give an answer to this question.

If, however, we suppose light to consist of material particles, endued with a mutually repulsive force, and so constituting an elastic fluid of great tenuity, the question just proposed may be solved without difficulty: for that such a fluid in passing through a narrow aperture, when pressed against an opposing surface, will be thrown into a series of undulatory movements, may be shown, I think, either by reference to the known laws of the motion of fluids, or by direct experiment. Take a tube, an inch or more in diameter, closed at one end,

excepting a small orifice or slit, and blow strongly through it against the flame of a candle. The flickering motion of the flame will show that the air does not issue from the orifice in an uniform stream, but in a succession of rapidly alternating movements. This fact was observed by Dr. Young, and described by him in a paper published in the *Philosophical Transactions* for 1800. The mode of observation that he employed was, indeed, somewhat different from that here described: he contrived to render the issuing current of air visible by combining it with smoke. If the pressure is increased, a whistling sound is produced; affording another kind of proof that the air in passing through the orifice is thrown into a state of undulation.

Now, if the particles of light be supposed to constitute an elastic fluid, it is evident that a phænomenon of the same kind must take place whenever a stream of this fluid impinges upon a narrow aperture; and in this case the formulæ of Fresnel will be applicable, with some modification, to the hypothesis here proposed, as well as to the hypothesis which supposes light to consist in the vibrations of an imaginary æther; and with this advantage, that an obvious reason is assigned why the undulations are found to have their origin at the aperture by which the light enters: for which no reason, as it appears to me, can be given on the opposite theory.

By the help of the same principle it might also be explained, I think, why a beam of light passing through a very narrow slit, is separated into two portions, leaving a dark space between them; and why the formulæ of Fresnel are found to fail in approaching this limit. But it would, perhaps, be out of place at present to enlarge on these questions, which are not likely to excite much interest while the theory on which they rest is not yet recognised or established. I could bring forward other considerations in abundance which appear to me decisive against the undulatory hypothesis; but it seems preferable thoroughly to investigate and discuss, in the first instance, the circumstances of one or two classes of phænomena, rather than to lose ourselves in a wider circle of reasoning.

I cannot conclude without again expressing my sense of the friendly spirit in which Professor Powell has criticized my former observations. I should be fastidious indeed, if I did not perceive and acknowledge that his remarks deserve to be considered in no other light than as intended, and as calculated, "to promote the common cause of scientific truth."

XXXIII. *On Bernoulli's Solution of the Problem of Shortest Twilight.* By T. S. DAVIES, Esq. F.R.SS. L. & E., F.R.A.S., &c.*

THE problem of the shortest twilight was first solved, and very elegantly too, by Nunez, in a little quarto tract of 142 pages, printed at Lisbon in 1542; and it has engaged the attention of many distinguished authors since that time. John and James Bernoulli as well as D'Alembert and L'Hôpital discussed the problem: but the researches of the two illustrious brothers are nowhere, that I know of, to be found in print. The result is, indeed, stated in one of John Bernoulli's letters to the *Journ. des Savans*: but he says nothing that can lead us to discover the particular details of his solution. He however certainly bestowed a good deal of trouble upon it; as he says, "*J'ai résolu le problème de trouver géométriquement le jour du plus petit crépuscule, ce qui a occupé mon frère, Professeur de Mathématique à Bâle, et moi, depuis plus de cinq ans, sans en pouvoir venir à bout.*" He tells us, too, that he effected his solution by means of the differential calculus. D'Alembert, in the *Encyclopédie Méthodique* (Art. CRÉPUSCULE), undertook to give a complete solution on the supposed plan of Bernoulli: but he obtained also an equation of the fourth degree, the roots of which embarrassed him considerably. Two of the roots, it is true, he dismissed from his formula pretty readily; but not, as it appears to me, upon grounds altogether satisfactory. He does not attempt to show either how they came there or what was their actual signification; nor does he even attempt to do so with the remaining root of the remaining pair, though he employs more than three folio columns in showing (by special instead of general reasonings) that it does not belong to the minimum, and he even says, and attempts to prove, that it does not refer to the maximum twilight problem.

The intricacy of D'Alembert's solution seems to have deterred successive geometers from attempting to develop in a more advantageous form the probable method of Bernoulli, these being the only solutions (L'Hôpital's perhaps excepted, of which I cannot speak, not having his book within reach,) which I have found, in which the modern method of maxima and minima is employed. Every other solution with which I am acquainted proceeds upon certain geometrical considerations derived from the nature of the figure itself rather than from the equations of the problem: and it must be admitted that in point of facility these have very considerably the ad-

* Communicated by the Author.

vantage over the other method. Had, therefore, the mere solution of the problem been the object to be principally sought for, there can be no doubt that it is already rendered as simple as it ever can be; and a very ample list of such solutions may be consulted in Professor Leybourn's edition of that valuable but unpretending little periodical, the *Ladies' Diary*, vol. iv. p. 314. We find there the solutions of Messrs. Ivory, Wallace, and Skene, as well as the very amplified discussions of Delambre, as given both in his *Astronomie*, and in his *Histoire de l'Astronomie Ancienne*. Two other solutions by Hachette, printed in the Correspondence of the Polytechnic School, vol. ii.; and others still, by Keill, Lemonnier, Maupertuis, Emerson, Mauduit, Cagnoli, &c., may be found in the respective writings of those authors. All these, however, are of the kind which may be called geometrical, in contradistinction to the method of Bernoulli and D'Alembert.

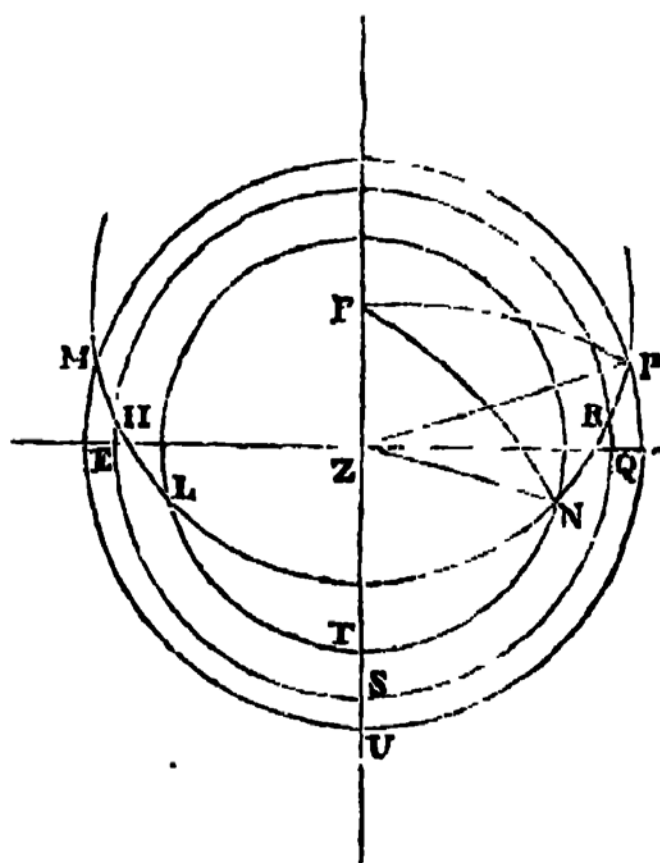
Of these, Mr. Skene's is the only one which takes the problem in the more general form, in which another almucantar is substituted for the horizon of the common problem; and his solution is truly elegant as a specimen of the method of research which he adopted. Unfortunately it confined him to the simple question of the minimum, and he also was thereby led to give an incomplete solution, compared with that furnished by the calculus. The two cases, the maximum and the minimum, are so intimately connected that they present themselves naturally as the extreme cases of the question, *When the duration of twilight is given, to find the declination*; and they appear together in the same final equation interwoven in a single formula, and demanding in all respects equal attention. On this account, elegant as the solution of Mr. Skene is, it does not remove the necessity for reconsidering the problem under another aspect.

Some time ago, when composing a paper (since published in vol. xii. of the *Edinburgh Transactions*), on the application of great-circle co-ordinates to the investigation of spherical loci, I was led to examine a considerable number of those problems which had been subjects of discussion, by the older methods of treating spherical curves—and the problem of shortest twilight amongst the rest. As, however, that problem did not properly appertain to my plan, I relinquished the inquiry for others more interesting to me at that moment, and laid aside the few memoranda which I had made concerning it. A day or two ago my attention was accidentally recalled to the subject; and as in those memoranda the problem had been viewed under its most general aspect, and the true character of all the roots of the equation assigned,—as, moreover, the

problem is one of some historical celebrity, and its solution calculated to enforce an important truth,—on all these accounts its occupying a place in the Philosophical Magazine may perhaps be justified.

PROBLEM.—Let EHRQ be the horizon of the place; Z the zenith; P the pole; ESQ the equator; MUP', LNT any two almaccantars; LNPM a circle of declination; then it is required to find the position or polar distance of LNPM, so that the arc of it intercepted between the given almaccantars may be a maximum or a minimum.

Such is the more general geometrical statement of the problem in the aspect under which the Bernoullis viewed it: the angle NPP' is the measure of the time elapsed between the sun's passing the two almaccantars. In the common problem for one of these almaccantars is substituted the horizon itself.



Let NP' be the intercepted portion of the circle of declination. Denote the colatitude, or PZ, by λ : put $\varrho, \varrho_1, \varrho_2$ the spherical radii of the circles LRN, LTN, and MUP' respectively: and denote the angles ZPP' and ZPN by θ_2 and θ_1 . Then,

$$\theta_1 = \cos^{-1} \frac{\cos \varrho_1 - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda}.$$

$$\theta_2 = \cos^{-1} \frac{\cos \varrho_2 - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda}$$

From which we have the polar angle or measure of the time,

$$\begin{aligned} \theta_2 - \theta_1 &= \cos^{-1} \frac{\cos \varrho_2 - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda} \\ &\quad - \cos^{-1} \frac{\cos \varrho_1 - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda} \dots\dots\dots (1) \end{aligned}$$

But that the time may be a maximum or a minimum, we must have

$$\begin{aligned} d \left\{ \cos^{-1} \frac{\cos \varrho_2 - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda} \right. \\ \left. \cos^{-1} \frac{\cos \varrho_1 - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda} \right\} &= 0 \dots\dots (2) \end{aligned}$$

In this expression ϱ is the variable, and the differentiation being performed with respect to it, we have, after transposition,

$$\frac{(\cos \lambda - \cos \varrho_I \cos \varrho) \operatorname{cosec} \varrho}{\sqrt{(\sin^2 \lambda - \cos^2 \varrho_I) + 2 \cos \varrho_I \cos \lambda \cos \varrho - \cos^2 \varrho}} \\ = \frac{(\cos \lambda - \cos \varrho_{II} \cos \varrho) \operatorname{cosec} \varrho}{\sqrt{\sin^2 \lambda - \cos^2 \varrho_{II} + 2 \cos \varrho_{II} \cos \lambda \cos \varrho - \cos^2 \varrho}} \dots (3)$$

Squaring these to take the radical from the denominator, and transposing all to one side, it takes the form

$$0 = \operatorname{cosec}^2 \varrho \left\{ \frac{(\cos \lambda - \cos \varrho_I \cos \varrho)^2}{\sin^2 \lambda - \cos^2 \varrho_I + 2 \cos \varrho_I \cos \lambda \cos \varrho - \cos^2 \varrho} \right. \\ \left. - \frac{(\cos \lambda - \cos \varrho_{II} \cos \varrho)^2}{\sin^2 \lambda - \cos^2 \varrho_{II} + 2 \cos \varrho_{II} \cos \lambda \cos \varrho - \cos^2 \varrho} \right\} \dots (4)$$

This expression is probably not much unlike that which John Bernoulli obtained, allowance being made for the greater generality of its objects and the difference of the notations. As, however, he merely describes it, we can only conjecture what it might have been*: at all events it does not greatly differ from that given by D'Alembert, in the *Encyclopédie Méthodique*, Art. CRÉPUSCULE. Let us now attend to the reduction of the factor within the brackets: and first, cross multiply, and arrange the resulting terms according to the powers of $\cos \varrho$; then divide all the terms by $\cos^2 \varrho_{II} - \cos^2 \varrho_I$: our result is

$$\cos^4 \varrho - 2 \cos \lambda \frac{1 + \cos \varrho_I \cos \varrho_{II}}{\cos \varrho_I + \cos \varrho_{II}} \cos^3 \varrho - \sin^2 \lambda \cos^2 \varrho \\ + 2 \cos \lambda \frac{1 + \cos \varrho_I \cos \varrho_{II}}{\cos \varrho_I + \cos \varrho_{II}} \cos \varrho - \cos^2 \lambda;$$

which may again be readily changed into

$$(1 - \sin^2 \varrho)^2 + 2 \cos \lambda \frac{1 + \cos \varrho_I \cos \varrho_{II}}{\cos \varrho_I + \cos \varrho_{II}} \cos \varrho (1 - \cos^2 \varrho) \\ - \sin^2 \lambda (1 - \sin^2 \varrho) - \cos^2 \lambda;$$

and this is at once converted into

$$\sin^2 \varrho \left\{ \cos^2 \varrho - 2 \cos \lambda \frac{1 + \cos \varrho_I \cos \varrho_{II}}{\cos \varrho_I + \cos \varrho_{II}} \cos \varrho + \cos^2 \lambda \right\} \dots (5)$$

Inserting instead of the bracketed factor of (4) its value given by (5), we have that equation (4) at once transformed into

$$\cos^2 \varrho - 2 \cos \lambda \frac{1 + \cos \varrho_I \cos \varrho_{II}}{\cos \varrho_I + \cos \varrho_{II}} \cos \varrho + \cos^2 \lambda = 0 \dots (6)$$

* Johan. Bernoulli, *Opera Omnia*, tome i. p. 64.

We thus see that the factor $\sin^2 \varphi$ which arose out of the process of finding (5) is really a *foreign factor* introduced, as those factors so generally are, by elimination: and we see too how it arose; viz. by Bernoulli and D'Alembert having cancelled the factor $\operatorname{cosec}^2 \varphi$ from equation (4) merely because it was a factor of the whole equation. 'Had this view occurred to them, they would have depressed their biquadratic to a quadratic at once, and have escaped the embarrassment created by attempting to prove that certain roots contained in it did not fulfill the conditions of the problem, and were *therefore* inadmissible. But an important question here arose—*what is the signification of this result, and how did it make its appearance intermingled with the proper solution of the problem?* It is not enough to take those parts of a result which we can readily interpret, and reject all the others as useless or unmeaning: though such is and always has been so greatly the fashion, that scarcely a single author has insisted upon an unflinching determination to consider every solution incomplete which stops short of this consummation. It will, I have no doubt, ere long, become a fundamental principle in the philosophy of mathematics—that *every part of an algebraical result admits of complete interpretation, either by reference to the conditions which were expressed in the fundamental equations, or else in the hypotheses, tacitly made (in order to apply our transforming operations,) in the various subsequent stages of the solution.* It is, however, so much the custom to discard, without consideration or remark, all such results as do not admit of a ready and obvious application to the immediate objects before us, that facility of interpretation is the rarest of all the faculties of the geometer that we find in any considerable degree developed. No factor ought to be rejected for which a satisfactory reason cannot be given, *nor ought it to be considered foreign till the step at which it was introduced, is distinctly ascertained:* and it bespeaks both a bad taste and unpardonably negligent habits of research, to leave any part of the final equation unexplained, even in the most trivial inquiry which may be undertaken*.

* The valuable discussion and illustrations of this principle, given by Mr. Babbage in vol. ii. of the Cambridge Philosophical Transactions, cannot be too earnestly recommended to every young mathematician who would form a proper taste in the modern analysis, nor too diligently studied by every one who aspires to a higher character than that of a mere algebraical machine.

With regard, however, to the metaphysical views entertained by that distinguished philosopher, on the relations between the mind and certain objects of mental action, as expressed in that paper, I offer no opinion *here*, as I shall have occasion to moot that subject in another place.

Resolving now the quadratic equation (6), we have

$$\begin{aligned}\cos \varrho &= \cos \lambda \frac{1 + \cos \rho_1 \cos \rho_{II} \pm \sin \rho_1 \sin \rho_{II}}{\cos \rho_1 + \cos \rho_{II}} \\ &= \cos \lambda \frac{1 + \cos \overline{\rho_1 + \rho_{II}}}{\cos \rho_1 + \cos \rho_{II}} \\ &= \cos \lambda \frac{\cos^2 \frac{1}{2} \overline{\rho_1 + \rho_{II}}}{\cos \frac{1}{2} \rho_1 + \rho_{II} \cos \frac{1}{2} \rho_1 \rho_{II}} \dots\dots\dots (7)\end{aligned}$$

But the question now naturally arose in D'Alembert's mind, which of these roots belongs to the *minimum*? or do they *both* designate *minima*?

It is difficult to account for that distinguished geometer overlooking the simple and common test furnished by the differential calculus itself for deciding this question: it is even difficult to conceive how the consideration of the problem itself, as presented by the diagram on the globe, should have failed to point out to him the proper interpretation of the two-fold result in our last equation: yet such is the case. He devotes considerable space to proving that the lower sign of our equation does not answer to a minimum twilight. What it does answer to he does not attempt to explain; but he denies that it answers to the maximum.

The general fact, that functions of one independent variable which do not admit of indefinite increase and diminution, can never have two maxima without the intermediate occurrence of a minimum, nor two minima without an intermediate maximum, might have guided him to discover from the figure itself, that the two roots could not both indicate minima; but rather that one of them necessarily indicated the time of *maximum twilight*. However, the common appeal to the second differential coefficient determines it at once. For differentiating (6), and inserting the values of $\cos \rho$ given in (7) in the equation, we have for the test the value of

$$\begin{aligned}\cos \lambda \frac{1 + \cos \rho_1 \cos \rho_{II}}{\cos \rho_1 + \cos \rho_{II}} \mp \cos \lambda \frac{\sin \rho_1 \sin \rho_{II}}{\cos \rho_1 + \cos \rho_{II}} \\ - \cos \lambda \frac{1 + \cos \rho_1 \cos \rho_{II}}{\cos \rho_1 + \cos \rho_{II}},\end{aligned}$$

the first and third terms of which cancel, and leave, for the test, the sign of

$$\mp \cos \lambda \frac{\sin \rho_1 \sin \rho_{II}}{\cos \rho_1 + \cos \rho_{II}} \dots\dots\dots (8)$$

We thus see that the problem has never yet been fully solved,

since the determination of the minimum depends not alone upon the choice of the signs + or — in the equation (7), but also upon the values of λ , ρ , and ρ_{II} . The way in which this takes place is too obvious to need any further remark.

The interpretation, therefore, of Bernoulli's equation is completed and the problem at the same time resolved in the most general case that can be proposed on a spherical earth, and with a uniform declination for the period of twilight on the day in question. It might hence seem that the inquiry, in reference to everything of value which it can afford, might be properly concluded here. Nevertheless, as the curious properties which Delambre has deduced, both in his *Astronomie* and in his *Histoire de l'Astronomie*, for the particular case he considered, have in several instances remarkable analogies in the more general one above discussed, it will not be out of place to annex a few of them to the preceding investigation.

[To be continued.]

XXXIV. *Theory of the Correction to be applied to a Ball-Pendulum for the Reduction to a Vacuum. By the Rev. J. CHALLIS, Fellow of the Cambridge Philosophical Society*.*

IN a previous communication respecting the resistance to the motion of small spherical bodies in elastic fluids†, I attempted to explain, entirely from theoretical considerations, the manner in which the air is acted upon, when a pendulum consisting of a small spherical ball suspended by a very slender wire, performs very small oscillations in it; but I omitted to enter upon any calculation to ascertain the numerical value of the correction required for reducing the time of vibration in air to that in a vacuum. As the theory there advanced is competent to obtain such a result without the aid of experiment, I propose to make this the object of the present communication.

The following equation was obtained in the paper referred to:—

$$M v^2 + m v^2 = 2 g (M - \mu) (h - x),$$

in which M is the mass of the ball, v the velocity of its centre, μ the mass of an equal volume of air, g the force of gravity, $h - x$ the vertical descent of the centre of the ball. The equation without the term $m v^2$, is that which was formerly em-

* Communicated by the Author.

† See Lond. and Edinb. Phil. Mag. vol. i. p. 40.

ployed in reducing to a vacuum, the effect of the motion of the fluid not being taken into account. According to our theory $m v^2$ is to be added, in consequence of the simultaneous movement of the air with the pendulum. The reasoning led to the conclusion that so little change of density takes place at the surface of the ball, that we may consider the air to be put in motion just as if it were incompressible. The influence of the air carried by the suspending wire was neglected in the theory. Also the surface of the ball was supposed to be perfectly smooth, so as to impress no motion on the aerial particles in the direction of a tangent plane. Hence the air in contact with the ball will move in directions normal to its surface, and consequently directed to a centre. Because the density is very nearly unchanged, the velocity at a given instant will very nearly vary at different points, in a radius produced, inversely as the square of the distance from the centre.

These results being admitted, we may proceed to calculate $m v^2$. For conceive two straight lines to be drawn at any instant through the centre of the ball, one in the direction of its motion, the other in any direction making an angle θ with the first. Let the plane of these two lines make an angle ϕ with a plane through the centre of the ball, at right angles to the suspending wire. The velocity of the air at the point where the latter line meets the surface is $v \cos \theta$; and at any point P on the line, distant by r from the centre, the velocity is $\frac{v b^2}{r^2} \cos \theta$, b being the radius of the ball. The mass of a fluid element at P, its density being 1, is

$$d r \times r d \theta \times r \sin \theta d \phi,$$

and the *vis viva* of the fluid in motion, or $m v^2$, is equal to

$$\iiint \left(\frac{v b^2}{r^2} \cos \theta \right)^2 r^2 \sin \theta d r d \theta d \phi.$$

The integral with respect to ϕ is to be taken from 0 to 2π , with respect to θ from 0 to π , and with respect to r from b to infinity. Hence

$$\begin{aligned} m v^2 &= b^4 v^2 \iiint \frac{d r}{r^2} \cos^2 \theta \sin \theta d \theta d \phi \\ &= 2\pi b^4 v^2 \int \frac{d r}{r^2} \cos^2 \theta \sin \theta d \theta \\ &= \frac{4\pi b^4 v^2}{3} \int \frac{d r}{r^2} = \frac{4\pi b^3 v^2}{3}. \end{aligned}$$

It follows that $m = \frac{4\pi b^3}{3} = \mu$, and that

$$Mv^3 + \mu v^3 = 2g(M - \mu)(h - x).$$

Hence $-\frac{v dv}{dx}$ or $f = g \frac{M - \mu}{M + \mu} = g \left(1 - \frac{2\mu}{M}\right)$ very nearly.

and if l' = the length of the seconds pendulum in air, l in vacuum,

$$\frac{l'}{l} = \frac{f}{g} = 1 - \frac{2\mu}{M}, \text{ and } l' = l \left(1 - \frac{2\mu}{M}\right).$$

The coefficient by which the old correction is to be multiplied, is consequently 2.

M. Bessel has obtained by experiment 1.956 for the value of this coefficient. The experiments of Mr. Baily (Transactions of Royal Society for 1832, p. 399*,) give 1.864 for spheres $1\frac{1}{2}$ inch in diameter, and 1.748 for spheres 2 inches in diameter, and consequently show that the coefficient is different for spheres of different diameter. No such difference is recognised either by the present theory, or by that of M. Poisson, who has taken into account the effect of the friction of the air against the surface of the ball. The theory of this difference, therefore, stands in need of further inquiry. Experiment is the most proper means of determining the *amount* of correction to be applied to the pendulum, but theory combined with it may enable us to determine the causes to which the correction is due.

Papworth St. Everard, Aug. 2, 1833.

XXXV. *Characters of some undescribed Genera and Species of Araneidæ.* By JOHN BLACKWALL, Esq. F.L.S. &c.†

Tribe, INEQUITELÆ, Latreille.

Genus, *Nerienne*.

EYES eight in number, unequal in size, disposed in two transverse rows on the anterior part of the cephalothorax; the intermediate eyes of both rows form a trapezoid whose anterior side is considerably the shortest; the lateral ones are placed obliquely, in pairs, each pair being seated on a small eminence and geminated; the posterior eyes of the trapezoid are larger, and the anterior ones much smaller than the rest.

Maxillæ strong, inclined towards the lip, slightly dilated at

* An abstract of Mr. Baily's paper will be found in Lond. and Edinb. Phil. Mag. vol. i. p. 379.—EDIT.

† Communicated by the Author; by whom other undescribed *Araneidæ* were characterized in our last Number.

the base where the palpi are inserted, and greatly so at the apex, which is obliquely truncated externally.

Lip short, broad, prominent at the apex, and semicircular.

Legs moderately long and robust; the anterior and posterior pairs, which are the longest, equal in length; the third pair is the shortest.

The spiders of this genus fabricate small horizontal sheets of web of a slight texture, among coarse herbage, or in cavities beneath stones, on the under side of which they take their station, in an inverted position, and watch for their prey.

Nerienne marginata.

Cephalothorax oval and glossy; the anterior part, on which the eyes are seated, elevated but obtuse; the posterior part depressed, with an indentation in the medial line. Mandibles strong, armed with teeth on the inner surface, and inclined towards the pectus which is heart-shaped. Maxillæ enlarged at their apices, and slightly inclined towards the lip. These parts are of a very dark brownish-black colour. Legs and palpi provided with erect spines; their colour is reddish-brown, with bands of a darker hue. Both of the superior tarsal claws are pectinated, and the inferior one is inflected near its base. A plain claw, slightly curved, terminates each palpus. Abdomen oval, rather convex above, projecting over the base of the cephalothorax; the upper part is brownish-black bordered by a dentated band of pale brown thickly spotted with white, which passes above the spinners, but whose continuity is interrupted in front by a black streak intersecting it at right angles; a series of curved, angular lines of a pale-brown colour, minutely spotted with white, extends along the middle; their convexity is towards each other, and their apices are directed forwards. Sides and under part of the abdomen brownish-black, marked with a few white spots, four minute ones, describing a large quadrangle, occurring on the latter. Plates of the spiracles dark red-brown.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{3}{20}$ ths of an inch; length of the cephalothorax $\frac{1}{4}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{3}$; length of an anterior leg $\frac{1}{3}$; length of a leg of the third pair $\frac{1}{6}$.

The male resembles the female in colour, and in the relative length of its legs, but the absolute length of those organs is greater, an anterior one measuring $\frac{1}{4}$ th of an inch. The third and fourth joints of the palpi are short, the latter, which is much the stronger, being fringed with long bristles on the outer side of the upper part; the fifth joint is oval, convex externally, concave within, comprising the sexual organs; they

are highly developed, complex with spiny processes, and are of a brownish-black colour tinged with red. Very old males are darker coloured and have red legs.

In the months of October and November this spider is common in the plantations about Crumpsall Hall, constructing its web among the grass under the trees.

Nerienne rubens.

Cephalothorax inversely heart-shaped, slightly convex above, and glossy, with a depression and an indentation in the posterior region; in front is an abrupt eminence, on which are the eyes, placed on black spots. Mandibles robust, conical, strongly toothed on the inner surface, and inclined towards the pectus, which is heart-shaped. Maxillæ moderately enlarged at their apices, and inclined towards the lip. Legs and palpi hairy, the former provided with a few spines; the two superior tarsal claws are pectinated, and the inferior one is inflected near its base. Palpi abundantly supplied with black spines, one longer than the rest, projecting from the anterior extremity of the third joint. These parts, with the exceptions already noticed, are of a yellowish-red colour. Abdomen oval, somewhat convex above, projecting over the base of the cephalothorax, thinly clad with hair, glossy, and of a red-brown hue, which varies in intensity in different individuals. Sexual organs black with a tinge of red. Plates of the spiracles pale orange.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{8}$ th of an inch; length of the cephalothorax $\frac{1}{16}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{18}$; length of an anterior leg $\frac{1}{6}$.

The male is rather smaller than the female, but the relative length of its legs is the same; the tibiæ of the first and second pairs are dilated underneath at their anterior extremities, and these enlargements are thickly clad with fine long hairs. The second joint of the palpi is very powerful, enlarging gradually from the base to its anterior extremity, which is armed with a strong spur, and a great number of minute, sharp-pointed, black spines, on the upper side; the third joint is short and robust; the fourth is furnished with two apophyses; the inner one is much the longer and tapers to a point: it is curved obliquely across the upper part of the fifth joint, and is provided with a strong projecting point near its base; the exterior apophysis is much shorter and more obtuse: the fifth joint is oval, convex externally, concave within, comprising the sexual organs, which are complicated in structure, exhibiting

several curved, spiny processes, and are of a dark red-brown colour.

Mr. T. Blackwall found this species among heath, in Trafford Park, near Manchester, in October 1832, at which season of the year it pairs.

Nerienne cornuta.

Cephalothorax glossy, inversely heart-shaped, depressed behind, with a large indentation in the medial line, and the sides are marked with furrows extending from the superior part to the margins; the anterior part is elevated and surmounted by two conical protuberances directed forwards, below which, in front, the eyes are situated. Mandibles strong, conical, perpendicular, and armed with teeth on the inner surface. Maxillæ inclined towards the lip, which they encompass. Lip short, prominent at the apex, and semicircular. Pectus convex and heart-shaped. These parts are of a very dark-brown colour, the anterior portion of the cephalothorax being almost black. The colour of the legs and palpi is light red-brown. The two superior tarsal claws are slightly pectinated, and the inferior one is inflected near its base. The fourth joint of the palpi is shorter than the third, and projects three apophyses from its anterior extremity, one on the inner side, which is long, curved, and pointed; a small one in front, which is also curved, and pointed; and one on the under side, which is short and obtuse; the fifth joint is oval, convex externally, concave within, comprising the sexual organs; they are highly developed, complex with spiny processes, and are of a dark red-brown colour. Abdomen oval, rather convex above, projecting over the base of the cephalothorax, sparingly supplied with short hairs, glossy, and black, with the exception of the plates of the spiracles, which are of a pale yellow colour.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{10}$ th of an inch; length of the cephalothorax $\frac{1}{20}$; breadth $\frac{1}{22}$; breadth of the abdomen $\frac{1}{21}$; length of an anterior leg $\frac{3}{20}$; length of a leg of the third pair $\frac{1}{9}$.

I captured males of this species in May 1833, on rails and gates at Oakland; but the female has hitherto escaped detection.

Closely allied to the spiders of the genus *Linyphia* by the disposition and relative size of their eyes, but approximating more nearly to those of the genus *Theridion* in the structure of the mouth and the relative length of their legs, the *Nerienne*

present another striking instance of propinquity between the *Inequitelæ* and *Orbitelæ*; a connexion which is further established by the mixed character of their œconomy.

Tribe, ORBITELÆ, } Latreille.
Genus, LINYPHIA, }

Linyphia minuta.

Cephalothorax glossy, inversely heart-shaped, prominent before, with an indentation in the medial line of the posterior region, and of a dark-brown colour. Pectus heart-shaped, and very dark-brown, approaching to black. Mandibles strong, vertical, and of a dark red-brown hue. Maxillæ straight, nearly quadrate, having the exterior angle at the apex curvilinear. Lip short, and semicircular. The colour of these organs is the same as that of the mandibles. Legs and palpi long, slender, and supplied with numerous delicate spines; they are reddish-brown with brownish-black bands. First pair of legs the longest, then the second, the third pair being the shortest; the two superior tarsal claws are pectinated, and the inferior one is inflected near its base. Each palpus is terminated by a single claw, slightly curved and pectinated. The four intermediate eyes form a trapezoid whose shortest side is in front; the other four are disposed in pairs on the sides of the trapezoid, the eyes constituting each pair being contiguous, and placed obliquely on an eminence; the two posterior eyes of the trapezoid are the largest, and the two anterior ones the smallest of the eight. Abdomen oval, convex above, projecting over the base of the cephalothorax, and thinly clad with hair; the upper part is pale-brown minutely spotted with yellowish-white; along the middle extends a series of strongly marked, brownish-black angular lines with their vertices directed forwards, and a little above the spinners is an irregular, transverse, semicircular line of a yellowish-white colour: the sides are brownish-black with a slightly curved line of yellowish-white, extending from the anterior part rather more than half-way towards the spinners; underneath, the abdomen is of a brownish-black colour, and the plates of the spiracles are yellowish-white.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{6}$ th of an inch; length of the cephalothorax $\frac{1}{4}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{3}$; length of an anterior leg $\frac{3}{10}$.

The body of the male is smaller and more slender than that of the female, but the legs are longer, an anterior one measuring $\frac{7}{20}$ ths of an inch. The third and fourth joints of the palpi are short and strong; the fifth has two conical pro-

cesses, or apophyses, on the upper part, near its articulation with the fourth joint; it is convex externally, concave within, comprising the sexual organs, which are of a red-brown colour.

I first observed this spider in little frequented rooms in Crumpsall Hall; it does not confine itself, however, to the interior of buildings, but may frequently be found on trees and shrubs, particularly such as grow against walls. In the month of September the female deposits thirty or forty spherical eggs, of a yellowish-white colour, in a cocoon of white silk, of a slight texture, and subglobose form, measuring about $\frac{1}{3}$ th of an inch in diameter.

Linyphia luteola.

This spider resembles *Linyphia minuta* in the form of the cephalothorax, except that the anterior part, on which the eyes are seated, is more prominent and acute; it is glabrous, and of a pale yellowish-brown colour, with black margins and a slender band of the same hue along the medial line. The eyes are placed on black spots, their arrangement and relative size being the same as in *Linyphia minuta*. The parallelism between the two species holds good also as regards the structure of the mandibles, the maxillæ, the lip, and the pectus. These parts are of the same colour as the ground of the cephalothorax. The legs are long and slender, the palpi rather short; both are provided with claws and delicate spines similar in structure to those of *Linyphia minuta*; their colour is a uniform pale yellowish-brown. The first pair of legs is the longest, then the second, the third pair being the shortest. Abdomen oval, compressed, remarkably convex above, projecting over the base of the cephalothorax; its colour is pale yellow, with minute white spots on the upper part; the sides are obscurely marked with oblique lines of a blackish hue, and above the spinners are several of a similar tint and an angular form. Plates of the spiracles yellow. In some individuals scarcely a trace of the black lines can be perceived.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{3}$ th of an inch; length of the cephalothorax $\frac{1}{12}$; breadth $\frac{1}{16}$; breadth of the abdomen $\frac{1}{10}$; length of an anterior leg $\frac{2}{3}$.

The male is smaller and more slender than the female, but the cephalothorax is longer, measuring $\frac{1}{10}$ th of an inch; it is very prominent and acute before, and is furnished with numerous strong, black bristles, particularly at the apex. The legs also are more elongated, an anterior one measuring $\frac{9}{20}$ ths of an inch. The maxillæ are exceedingly convex externally,

immediately above the insertion of the palpi; the second joint of the latter organs is robust; the third and fourth joints are very short, a strong bristle, rough with projecting points on the under side, depending from a prominence on the upper part of the anterior extremity of the former; the fifth joint is oval, convex externally, concave within, comprising the sexual organs, which are complex, with a curved pointed projection on the upper part; their colour is red-brown.

This species of *Linyphia* is plentiful in September and October, in the plantations about Crumpsall Hall, constructing, among the coarse grass beneath the trees, a horizontal web, three or four inches in diameter, on a plan similar to that employed by the other species of the genus. Like them, too, it is usually found on the under side of the horizontal sheet in an inverted position.

Tribe, CITIGRAPHÆ, Latreille.

Genus, *Hecaërge*.

Eyes eight in number, unequal in size, disposed in two transverse rows on the anterior part of the cephalothorax; the four constituting the anterior row, which is slightly curved backwards, are adjacent and minute, the two lateral ones being the smallest of the eight; the posterior row is greatly curved, with its convexity directed forwards; it comprises the other four eyes, which are large and separated by moderately wide intervals.

Maxillæ short, strong, convex on the under side, enlarged at the base where the palpi are inserted, rounded at the extremities, and inclined towards the lip.

Lip small, as broad as long, triangular, truncated at the apex.

Legs powerful; the fourth pair is the longest, then the first, the third pair being the shortest.

The spiders belonging to this genus are erratic, taking their prey by surprise.

Hecaërge maculata.

Cephalothorax oval, convex above, depressed in the posterior region, pointed before, and covered with short hairs; its colour pale yellowish-brown, with a broad dark-brown band extending along each side of the medial line, and two fine lines of the same hue on the lateral margins; a few longish white hairs, directed forwards, occupy the space between the two intermediate pairs of eyes, and immediately below the anterior row of eyes is a small transverse parallel line of a

dark red-brown colour. Mandibles small, conical, perpendicular, armed with a curved red nail at the extremity, and a few minute teeth on the inner surface; they are of a pale yellowish-brown colour, with a dark-brown elongated spot in front of each, extending from the base towards the extremity; this spot is palest in the medial line. Maxillæ pale yellowish-brown. Lip dark-brown bordered with pale-brown. Pectus heart-shaped, of a pale-yellow colour faintly tinged with green; eight dark-brown spots occur on its margins; one, which is very minute, and in some individuals wanting altogether, is situated opposite the lip; three are disposed on each side, and one is seated on its posterior extremity. Legs hairy, furnished with strong spines; thighs and terminal joint of the tarsi pale yellowish-brown, the former having two or three longitudinal lines of a dark-brown colour on the upper and outer sides, which are most conspicuous on the first and second pairs, and some minute spots of the same tint underneath; tibiæ and superior tarsal joint dark-brown, those of the first and second pairs of legs having a series of long, moveable, sessile spines on each side of the inferior part. The two tarsal claws are long, curved, and provided with three minute teeth, the terminal one being the longest; beneath them is a small brush, which enables the spider to ascend with facility smooth perpendicular surfaces. Palpi pale yellowish-brown, furnished with a few spines, two of which, on the under side of the fifth joint, are opposite and erect; a long curved claw, having three exceedingly small teeth underneath, terminates each palpus. Abdomen thickly covered with hair, oval, convex above, projecting over the base of the cephalothorax; upper part pale yellowish-brown mingled with white; three small tufts of white hair occur in front, near the cephalothorax; two narrow, obscure, brownish-black streaks, one on each side of the medial line, extend from the anterior part of the abdomen almost a third of its length, and are followed by a series of brownish-black spots, occupying the remainder of the medial line to the spinners, on each side of which is a longitudinal row of very small spots of the same hue; these streaks and spots are comprised between two irregular brownish-black bands composed principally of spots, confluent or nearly so, diminishing in size as they approach the spinners; sides and under part of the abdomen pale yellowish-brown spotted with brownish-black, the spots on the latter minute. Plates of the spiracles yellowish-brown with pale inner margins.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{4}$ th of an inch; length of the cepha-

lothorax $\frac{1}{10}$; breadth $\frac{1}{12}$; breadth of the abdomen $\frac{1}{10}$; length of a posterior leg $\frac{3}{8}$; length of a leg of the third pair $\frac{1}{4}$.

The male though smaller than the female is similar to it in colour, and in the relative length of its legs. The third and fourth joints of the palpi are short, a small pointed apophysis projecting from the outer side of the anterior extremity of the latter; the fifth joint is oval, convex above, concave beneath, comprising the sexual organs, which are highly developed, with a curved spiny process extending to the termination of the joint, and are of a red-brown colour.

This species, which I discovered in the woods about Oakland, in Denbighshire, in the month of April 1833, approximates most nearly in its general structure to the spiders of the genera *Lycosa* and *Dolomedes*; by the organization of the feet, however, it appears to connect the *Citigradæ* with the *Laterigradæ*. In the month of June the female constructs a lenticular cocoon of white silk, of a slight texture, measuring about $\frac{3}{10}$ ths of an inch in diameter, which she usually attaches to the under side of a stone, depositing in it between twenty and thirty spherical eggs of a yellowish-white colour, not agglutinated together.

Tribe, TUBITELÆ, Latreille.

Genus, *Erigone*, Savigny.

Erigone atra.

Cephalothorax inversely heart-shaped, very prominent before, with an indentation in the medial line of the posterior region. Mandibles powerful, rather convex in front, strongly toothed on the inner surface, and slightly inclined towards the pectus, which is heart-shaped. Maxillæ greatly enlarged at the base where the palpi are inserted, encompassing the lip, which is short and semicircular. Fourth pair of legs the longest, then the first, the third pair being the shortest. The two superior tarsal claws are pectinated, and the inferior one is inflected near its base. Eyes disposed in two transverse rows on the summit of the anterior convexity of the cephalothorax; the intermediate ones of both rows form a square, and the other four are placed obliquely in pairs, one on each side of the square. Abdomen oval, convex above, projecting over the base of the cephalothorax. This spider is sparingly clad with hair, and glossy black, with the exception of the legs and palpi, which, in adults, are of a reddish-brown colour, and of the plates of the spiracles, which are yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{9}$ th of an inch; breadth of the abdomen $\frac{1}{20}$; length of a posterior leg $\frac{1}{8}$.

The male differs from the female in several remarkable particulars; it is smaller, and presents, exclusively, some of those peculiarities of structure on which the generic character is established. The lateral margins of the cephalothorax are strongly dentated, and a series of short sharp spines, slightly curved forwards, occupies the medial line of its anterior convexity; each mandible also has a longitudinal row of spines, curved downwards, on its exterior side. (Small spines occur sometimes on the mandibles of old females.) The second joint of the palpi is greatly elongated, much curved, and is armed with strong spines beneath; the third and fourth joints are dilated at their anterior extremities, the former having a large conical apophysis on the lower side, and the latter two terminal apophyses, one short and broad projecting in front, the other more elongated and acute situated underneath; the fifth joint is oblong-oval, convex externally, concave within, comprising the sexual organs, which are highly developed, and of a very dark red-brown colour. The relative length of the legs likewise is different in this sex; the first pair is the longest, measuring $\frac{1}{4}$ th of an inch, and the fourth, second, and third pairs decrease successively in longitudinal extent. These organs and the palpi are much redder than those of the female.

Independently of colour, the spider described above may be distinguished by its external structure from the *Erigone vagans* of M. Savigny. (*Description de l'Égypte, seconde édition*, tome xxii. p. 319—321; *Atlas de Zool.*; *Arachn.* pl. i. fig. 9.) The male of the latter species has three rows of spines on the anterior convexity of the cephalothorax; a single row on the under part of each thigh of the first pair of legs, and the second pair of legs is longer than the fourth; whereas the new spider has only one row of spines in the former situation, and none in the latter, a few very minute ones merely occurring on the inner side of the thighs of the fore legs, near their base, and the fourth pair of legs is longer than the second; moreover, each palpus of the female of *Erigone vagans* has a pectinated claw at its extremity, but the palpi of *Erigone atra* are destitute of claws.

This diminutive spider is very plentiful and decidedly diurnal. Endowed with an instinctive impulse to ascend into the atmosphere, it frequently takes aërial excursions, and is active even in winter, when the weather is mild. It pairs in June, and deposits its eggs, which are large in proportion to the size of the animal, six or seven in number, spherical, not agglutinated together, and of a pale yellowish-white colour, in a cocoon of white silk, of a slight texture, and subglobose form,

measuring about $\frac{1}{10}$ th of an inch in diameter; it is usually placed under stones and in crevices of the earth.

There must be something very remarkable in the internal as well as external organization of this extraordinary spider, for numerous specimens of both sexes submerged in cold water on the 21st of October 1832, remained in that situation till the 22nd of November, an interval of 768 hours, without having their vital energies suspended. It is evident, therefore, that this species possesses the power of abstracting respirable air from water, for though in the act of submersion the spiracles are usually enveloped in a bubble of air, yet so small a supply must soon be exhausted, and, indeed, it speedily disappears.

Oakland, near Llanwrst, Denbighshire,
July 29, 1833.

XXXVI. *Additional Note on Conical Refraction.*
By J. MACCULLAGH, F.T.C.D.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE introductory part of my note which appeared in your last Number was written in haste, and I have reason to think it may not be rightly understood. You will therefore allow me to add a few observations that seem to be wanting.

The principal thing pointed out in the paper that I published some time ago in the Transactions of the Royal Irish Academy, is a very simple relation between the tangent planes of Fresnel's wave surface and the sections of two reciprocal ellipsoids. Now this relation depends upon the *axes* of the sections, and therefore naturally suggested to me the peculiar cases of circular section in which every diameter is an axis. Thus a new inquiry was opened to my mind. And accordingly, without caring just then to obtain final results, which seemed to be an easy matter at any time, I expressed in conversation my intention of returning to the subject of Fresnel's theory, in a supplementary paper. The design was interrupted, and I was prevented from attending to it again, until I was told that Professor Hamilton had discovered cusps and circles of contact on the wave surface. This reminded me of the cases of circular section, and the details given in my last note were immediately deduced.

I am, Gentlemen, &c.

J. MACCULLAGH.

Trinity College, Dublin, August 16, 1833.

XXXVII. *A Catalogue of Comets. By the Rev. T. J. Hussey, A.M. Rector of Hayes, Kent.*

[Continued from p. 102.]

[The Chronology employed is that of Petau or Petavius.]

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1759. E, that of 1661. F, that of 1677.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
171	673	...			Vita St. Leod. Centur. Chron. &c.	
172	676	...	January	Virgo.....	Chinese Records.	
173	—	...	Aug. Sept.....	Gem., Ur. Maj.	Anast. Paul. Diac. Chin. Records.	Seen 3 months.
174	681	...	Oct. Novemb.	Hercul. Aquila	Chinese Records.	Seen 17 days.
175	683	...	April, May	Auriga, Taurus	Chinese Records.	
176	684	...	Sept. Oct.	Tow ^d the West	Chinese Records.	
177	—	B	December	Taurus	Anast. Paul. Diac. &c.	
178	707	...	November.....	Tow ^d the West	Chinese Records.	
179	708	...	March	Musca, Taurus	Chinese Records.	
180	—	...	September		Chinese Records.	
181	711	...			Italy in Centil. Ptolem.	Continued 11 nights with a visible motion.
182	712	...	August.....	C. Beren., Leo, Virgo, Bootes	Chinese Records.	
183	716	...			Sabell. Bizar. &c.	
184	729	F?			Bede. Anast. &c.	
185	730	...	Aug. Sept.....	Auriga, Taurus	Chinese Records.	
186	738	...	April	Ursa Major ...	Chinese Records.	
187	744	...			Theoph. Cedren.	
188	760	...	May	Taurus, Gemini, &c. ...	Chinese Records.	Seen 59 days.
189	—	...	May, June.....	To the West	Chinese Records.	
190	762	...			Theophan. &c.	
191	767	...	January	Delphinus	Chinese Records.	
192	770	...	May	Auriga, Taurus	Chinese Records.	
193	—	...	June, July	Auriga, Camel.	Chinese Records.	
194	773	..	January	Orion	Chinese Records.	
195	813	...	August		Theoph. Sim. Logoth. Zonar, &c.	
196	815	...	April	Leo	Chinese Records.	
197	817	...	February	Sagittarius.....	Chi. Rec. Eginh. Aimoin, &c.	
198	821	...	February	Crater, Leo.		

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
199	—	...	July	Taurus.		
200	837	...			Chinese Records.	Elements computed by Pingré*.
201	—	...	September....	Aquar. Equul.	Chi. Rec. Boeth.	
				Pegasus....	Chinese Records.	
202	838	...	November....	Corvus.....	Chinese Records.	
203	—	...	Nov. Dec....	Sagitt. Scorpio	Chinese Records.	
204	839	...	February	Aquarius.....	Chin. Rec. Annal. Bertin.	
205	—	...	March, April..	Perseus	Chin. Rec. Annal. Bertin.	
206	840	...	March, April..	Pega. Androm.	Chinese Records.	
207	—	...	December	In the East ...	Chinese Records.	
208	841	...	June, July	Sagitt. Aquar.	Alber. Casin. Chin. Rec.	
209	—	...	December to February 842	Pisces, Pegas.	Chinese Records. Nith. Hist.	
210	852	...	March.....	Orion	Chinese Records.	
211	855	C			Chron. Malleac.	Seen during 20 nights.
212	857	...	September	Scorpio	Chinese Records.	
213	858	...	April.....	In the East ..	Ptolem. Hist. Martin. Fuld.	
214	864	...	June.....	Aries.....	Chron. Floriac. Chinese Rec.	
215	866	...	April		Const. Porphy.	
216	868	...	January	Aries, Musca..	Chinese Records. Ann. Fuld. &c.	Seen for about 17 nights.
217	869	...	September....	Pers. Cap. Med.	Chi. Ann. Pontan.	
218	873	...			Chron. Andega.	
219	875	...	June.....	Aries	Ann. Fuld. Chro. Casauv. &c.	
220	877	...	March	Libra.....	Chron. Novalic.	Seen 15 nights.
221	882	...	January.....		Annal. Fuld. Aventin. &c.	
222	885	...		Betw ⁿ Perseus and Gemini	Chinese Records.	
223	886	...	June.....	Scorp. Sagitt. U. Ma. Bootes.	Chinese Records.	

* Passage through the perihelion in mean time at Greenwich: March 0^d 23^h 51^m 0^s.—Long. of the perihel. on the orbit of the comet, 9° 19' 3" 0".—Long. of the ascending node, 6° 26' 33" 0".—Angle between the perihel. and the node, 9° 7' 30" 0".—Inclination of the orbit, 10° or 12°.—Perihel. distance, 0.580000.—Log. of the mean motion, 0.314986.—Direction R.

The angle between the perihelion and the node having been left out of the elements of the four comets published in the last Number, the omission is here supplied.	No.	Year.	2° 22' 0"
	50	240	8 }
	135	539	2 } 15 30
	139	565	2 10 0
	140	—	2 19 30

[To be continued.]

XXXVIII. Contributions to the Geology of Northumberland and Durham. By N. J. WINCH, Esq. F.G.S. & A.L.S.

[Continued from p. 99.]

An Account of Strata sunk through in the Blossom Pit, Hetton Colliery, begun December 19, 1820, Durham.

	F.	Y.	F.	I.	F.	Y.	F.	I.
Outset above the surface	1	0	0	0				
Soil	0	0	2	0				
Sand	1	1	0	0				
Gravel	0	1	1	0				
Limestone marl and soft limestone ...	3	0	0	0				
Limestone of various kinds, and the water increased gradually, from about the depth of 16 fathoms (where the machine began to pump,) to the bottom of it, to nearly 2000 gallons per minute; viz.								
— Yellow limestone in different beds .	15							
— Blue ditto and flaggy near the bottom (called blue rag)	7							
Blue metal	0							
Sandstone when broken into was drawn by the pumps	0							
White metal stone, very soft	0							
Gray scamy metal stone	0							
Strong brown limestone mixed with whin .	1							
Blue gray metal mixed with water and very soft	5							
Coal mixed with black metal	0							
					38	1	1	4
Gray metal stone								
Blue metal								
Coal mixed with black metal								
					2	1	0	5
Gray metal stone and water	1	2	10					
Section of the 4-feet Coal Seam.								
Coal	0 ^{y.}	1 ^{ft.}	0 ⁱⁿ					
Gray metal	0	0	4					
Black ditto with coal	0	0	7					
Coal	0	1	10	0	1	0	9	
					3	1	0	7
Gray metal stone or thill	0	1	4					
Gray ditto with partings	0	0	10					
Strong white post, and water in it, near the bottom, and spongy	0	1	8					
Three-quarter coal seam and water in it ...	0	2	4					
					8	0	0	2
Gray metal stone or thill				1				
White post								
Gray metal								
White post with partings and water								
Carried forward	8	1	0	5	2	1	2	6

	Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Brought forward ...	8	1	0	5	52	1	2	6
Gray metal stone ...	0	0	2	8				
Coal mixed with black metal ...	0	0	1	6				
					9	0	1	7
Thill ...	0	0	0	8				
White post ...	0	0	2	5				
Gray metal ...	0	0	2	2				
Gray metal stone with post girdles ...	0	0	1	4				
Gray metal ...	0	0	0	6				
Coal ...	0	0	0	10				
					1	0	1	11
Thill ...	0	0	0	3				
Gray metal stone ...	0	0	0	10				
White post with wet partings 8 feet down ...	1	1	1	2				
Gray metal stone mixed with post ...	0	0	2	9				
Black and blue metal ...	1	1	2	10				
Coal ...	0	0	0	3				
					4	0	2	1
Gray metal stone ...	0	0	1	3				
White post ...	1	0	1	9				
Gray and white post mixed with gray metal stone ...	1	1	2	2				
Whin ...	0	1	0	7				
White post and partings of blue metal and water wedged off ...	0	0	2	10				
Strong white post with a parting at bottom and water ...	1	1	1	6				
Strong white post and spongy with partings and water ...	4	1	2	11				
Gray and white post and the water wedged off ...	3	0	1	3				
Gray metal stone ...	0	1	1	6				
Coal ... 0y. 0ft. 4in.								
Gray metal ... 0 1 0								
Coal ... 0 1 3								
Band ... 0 0 5								
Coal ... 1 0 2								
Band ... 0 0 4								
Coal mixed with black metal ... 0 0 11	1	0	1	5				
					16	0	2	2
Thill ...	0	0	1	8				
Gray metal stone with large balls of iron-stone ...	1	0	2	4				
Strong gray metal with girdles ...	3	1	0	9				
Black metal with scares of coal ...	0	0	0	9				
Coal ...	0	0	1	2				
					5	1	0	8
Black gray metal with scares of coal ...	0	0	2	8				
Dark gray metal with balls of ironstone ...	0	0	2	6				
Strong gray metal stone with strong dunish post girdles ...	3	0	1	10				
Gray metal ...	1	0	0	8				
Five quarter coal seam ...	0	0	1	7				
					5	1	0	3
Carried forward ...					94	4	2	11

	Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Brought forward ...					94	4	2	11
Black metal with scares of coal	0	0	0	6				
Dark gray metal	0	1	1	4				
Coal	0	0	0	5				
Gray metal stone with post girdles	5	1	1	6				
Gray post	0	1	1	10				
Blue and gray metal stone	3	1	2	9				
Do. . do. with strong girdles	1	1	2	3				
Section of the High main coal seam, on the Wear, or Yard coal seam, on the Tync.								
Top Coal 0 ^{y.} 1 ^{ft.} 8 ^{in.}								
Band 0 0 1								
Coal 1 1 9								
	1	0	0	6	14	0	2	1
Total depth to the Main coal seam sunk } through 3rd September 1822 }					109	1	1	3

An Admeasurement of the Minor Pit, (which is 45 yards to the West of the Blossom Pit), to the Main Coal Seam and Strata sunk through from there to the Hutton Coal Seam, begun December 23, 1820.

	Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Total depth of the Minor Pit to the thill of the Main Coal Seam, sunk through various strata similar to those specified in the Blossom Pit sinking account, as above					108	1	1	1
White post	0	0	0	8				
Gray and white post	1	1	2	0				
Black metal stone	0	0	1	4				
Blue metal with iron stone girdles	1	0	2	4				
Black metal	0	0	1	4				
Gray metal stone	0	0	2	10				
Coal	0	0	0	5				
					4	0	1	11
Strong gray metal stone	0	0	2	0				
Gray and white post	4	1	0	9				
Gray metal	1	1	0	2				
Coal	0	0	1	2				
					6	1	1	1
Gray metal	0	1	0	9				
Coal	0	0	0	4				
					0	1	1	1
Gray metal	0	1	1	2				
Blue metal	0	1	1	0				
Gray metal	1	1	2	6				
Blue metal with coalpipes	0	1	0	8				
Strong gray post	0	1	2	8				
Do. do. with strong girdles	0	1	2	2				
Whin	0	1	0	0				
Soft gray metal	2	1	2	8				
Carried forward	9	0	0	10	120	0	2	2

	Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Brought forward	9	0	0	10	120	0	2	2
Low main coal seam, on the Wear, or Five- quarter coal seam, on the Tyne }	0	1	1	1				
					9	1	1	11
White post	2	0	0	6				
Whin	0	0	2	2				
Gray metal with post girdles	2	0	0	10				
Black and blue metal stone	1	0	0	0				
Coal	0	0	1	0				
					5	1	1	6
Gray metal with post girdles	0	1	2	11				
Coal 0y. 0ft. 4in								
Band 0 0 1½								
Coal 0 0 7								
Thill 1 1 0								
	0	1	2	0¾				
Strong gray metal	1	0	2	0				
Gray post	1	0	2	4				
Black metal	0	1	0	0				
Blue metal with post girdles	0	0	1	4				
Strong white post	4	0	0	0				
Gray metal and post girdles	1	0	1	0				
Section of the Hutton coal seam, on the Wear, or Low main coal seam, on the Tyne.								
Coal 1y. 1ft. 7½in								
Band 0 0 3½								
Bottom Coal (coarse) 0 1 3								
	1	0	0	2				
Total depth to the Hutton seam, or Low main seam of the collieries on the Tyne }					11	0	2	9¾
					147	0	2	4¼

The colliery, or rather the three collieries, in the neighbourhood of Hetton in the county of Durham, is at present the most important mining concern in this coal-field. These pits are said to be capable of working two hundred thousand Newcastle chaldrons of coals annually: they are situated about seven miles from Sunderland, to the south-west, and the shafts are sunk through the magnesian limestone, which here covers the coal-measures. The following extract of a letter from a professional man, on the subject of these mines, is worthy of insertion.

“ A bed of soft sandstone* was found under the magnesian limestone, abounding with water, in the Hetton and Elemore Pits; but at Eppleton sinking, no water was met with till near the bottom of the sandstone. It may be remarked that at the Hetton Pits there is twenty-six fathoms of limestone, and only four feet four inches of sandstone; at Elemore twelve and a

* This is the soft yellow sandstone which intervenes between the magnesian limestone and the coal-measures, so ably described by Prof. Sedgwick in the Geological Transactions, in his papers on the subject read in 1826, 1827 and 1828. [Abstracts of these papers were given in the Phil. Mag. and Annals, N.S. vol. i. p. 66, and vol. iii. p. 301.—EDIT.]

half fathoms of limestone and ten fathoms and a half of sandstone; and at Eppleton only nine and a half fathoms of limestone and twenty-one fathoms of soft sandstone. The Elmore Pits are one mile south-west of the Hetton Pits, and the Eppleton about one mile north-east of the Hetton Pits." In this part of the district the dip of the coal beds is towards the east, at the rate of one yard in twenty-two; but along the coast of Northumberland, and as far south as Manor Walls End*, at the mouth of the Tyne, they rise rapidly to the east, as well as more gently towards the west. On the other hand the coal-measures south of the river Wear continue to dip regularly to the eastward. Even at Hetton the high main coal had not recovered the good quality it was formerly known to possess on the Tyne; but the low main, a very inferior coal, from being brittle and breaking into small fragments before reaching the London market, is at Hetton and other Wear water collieries a coal of very superior quality. In all these mines the *low main of the Tyne* is called the *Hutton seam*. See section of Byker St. Anthon's Colliery, Geological Transactions, first series, vol. iv. pp. 41, 42.

[To be continued.]

XXXIX. *Remarks on one of Mr. Talbot's proposed Philosophical Experiments.* By C. WHEATSTONE, Esq.

To Sir David Brewster.

Dear Sir,

IN the last Number of the Philosophical Magazine is inserted an article, entitled "Proposed Philosophical Experiments, by H. F. Talbot, Esq. M.P. F.R.S." Upon a part of this communication, headed "On the Velocity of Electricity," I feel it necessary to make the following observations.

My experiments were not made solely with a view to determine the velocity of the electric spark *passing through air* as Mr. Talbot has inferred, but, from the first, were intended to extend to the passage of electricity through solid conductors. In fact, the very first experiment of the kind which I made, and which was shown to the members of the Royal Institution on the evening my investigations on this subject were first made public by my friend Dr. Faraday, was one by which I endeavoured to show the deviation from a vertical line of two sparks simultaneously visible at the opposite ends of a metallic conductor. Beyond this, I have for several months past communicated very generally to my scientific acquaintance the de-

* Almost all coals with us are now designated Walls End, whatever seams may produce them.

tails of an experiment upon a larger scale, which I hope soon to have an opportunity of executing, and which will detect and measure the velocity of electricity in its passage through a metallic conductor, though the rapidity of its transmission may exceed that of light: this I have proposed to effect, by increasing, in certain proportions, 1st, the velocity of the revolving mirror; 2ndly, the length of the conducting wire; and 3rdly, the accuracy of observing the deviation of the sparks from a vertical line. If I succeed in this point, it is obvious that we shall possess a means of directly measuring the relative conducting powers of metals, and of ascertaining numerous particulars respecting ordinary electricity which we at present have no means of determining.

Intending in the ensuing session to submit to the Royal Society all the results I have obtained, in reference to *a new optical means of measuring rapid motions, minute intervals of time, and feeble intensities of light*, I have hitherto refrained from publishing any incomplete statement of them; but I regret that this delay should have occasioned my experiments to be so far misunderstood, that one of the earliest which suggested itself to me, and which I have always considered to be of primary importance in the series, should be proposed elsewhere, several months afterwards, as an experiment yet to be tried, and be represented also as having entirely escaped my attention. I remain, Dear Sir, yours, &c.

Conduit-street, Hanover-square,
August 2, 1833.

C. WHEATSTONE.

*XL. Experimental Contributions towards the Theory of Thermo-electricity. By Mr. JOHN PRIDEAUX, Member of the Plymouth Institution.**

THE discovery of Professor Seebeck, that a bar of antimony, or of bismuth, heated in contact with a copper or brass wire, would affect the magnetic needle, was soon extended by chemists in this country and on the Continent to all the other metals which are of ready access; and a table of the thermo-electric order of these metals was soon published by Professor Cumming, and found to differ from the voltaic order, and also from that of conduction,* whether of heat or electricity.

Other experimenters presently discovered thermo-electric currents in single masses of metal, which have been traced out with curious results†.

* Communicated by the Author.

† [Papers on the thermo-magnetism of homogeneous bodies, by Mr. Sturgeon, will be found in *Phil. Mag. and Annals*, N.S. vol. x. pp. 1, 116, &c.—EDIT.]

M. Becquerel is, I believe, the only one who has investigated the comparative force as well as the order of arrangement of metals when acting in pairs; and having ascertained these points by a course of delicate experiments, he has been led to the inference that thermo-electricity is allied to the radiation of caloric.

The deep interest attached to a subject in which the mutual reaction of heat, electricity and magnetism is made almost tangible within a very narrow compass, led me to institute the following inquiries, with the hope of their suggesting other methods of elucidation.

I. Is thermo-electricity different from that derived from other sources: i. e. a different principle, or a different combination of principles?

II. Is it produced at the *expense* of caloric?

III. Is the radiation of heat, or any property dependent upon it, the proximate cause of thermo-electricity?

IV. Is the proximate cause connected with the conduction of heat?

V. Will not a hot bar brought into contact with a cold one of the same metal set electricity in motion; and if so, will the currents so produced bear any peculiar relation to those produced by pairs of different metals?

I. *Is there any, and what, difference between Thermo-electricity and that derived from other sources?*

1. The most characteristic distinction between electricity and the other imponderable fluids (if such they be,) is, its [sensibly] instantaneous transmission through considerable length of solid conductors. The very low tension of thermo-electricity renders this as close a test as any to which it can be subjected. Fifty feet of iron wire (one of the worst metallic conductors,) was cut into two lengths, through which (turned at the ends, to ensure metallic contact,) two other mercury boxes were connected with those of the magnetest*. A thermo-electric pair, of antimony and bismuth, heated at the point of contact, had their feet dipped, first into the mercury boxes of the magnetest, which produced a deflection of 80° ; and were then removed to the other pair of boxes at the end of the wires, by which the deviation was reduced to 15° ; and so repeatedly; the interposition of such a length of iron wire, between the excited metals and the magnetest, withholding $\frac{4}{5}$ ths of the deviation (about $\frac{1}{2}\frac{9}{10}$ ths of the current): yet was the

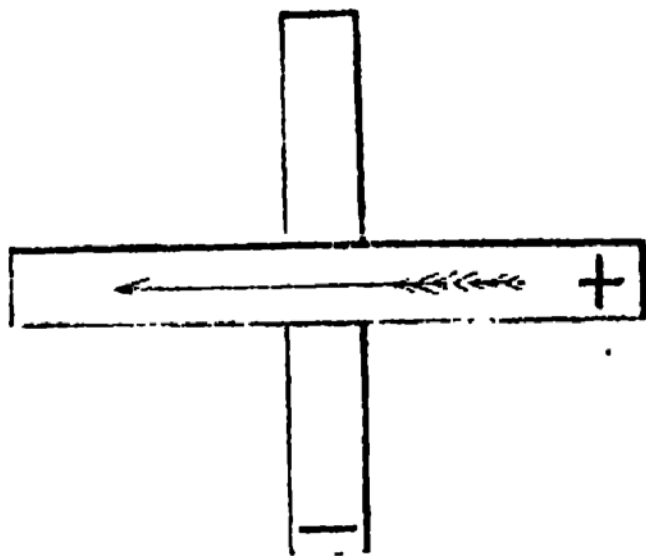
* I must be excused for resuming the use of this word: "galvanometer" would be hardly applicable in this instance.

instantaneous movement of the needle as evident in one case as in the other. So far, then, as promptitude of transmission through long wires is a distinction, thermo-electricity does not differ from the other kinds.

The magnetest employed in these experiments consists of a pair of sewing needles, 3 inches long, the lower one having just enough of predominance to give them terrestrial direction. The conductor, which passes only once between them, and returns beneath the lower needle, consists of four lengths of bright copper bell-wire laid together, not twisted, the ends amalgamated, and working into mercury boxes. A pair, of copper and tinned iron, by the heat of the fingers gives a deflection about 25° .

A guide-cross lies between the mercury boxes, to keep always before the eyes the relations between the current and the needle.

It is constructed of two slips of card, fastened in the centre, one over the other, with sealing-wax; one representing the magnet, the other the conducting wire. On the latter an arrow is drawn (on both sides) to indicate the direction of the current; on the former a line, also on both sides, to distinguish the marked pole.



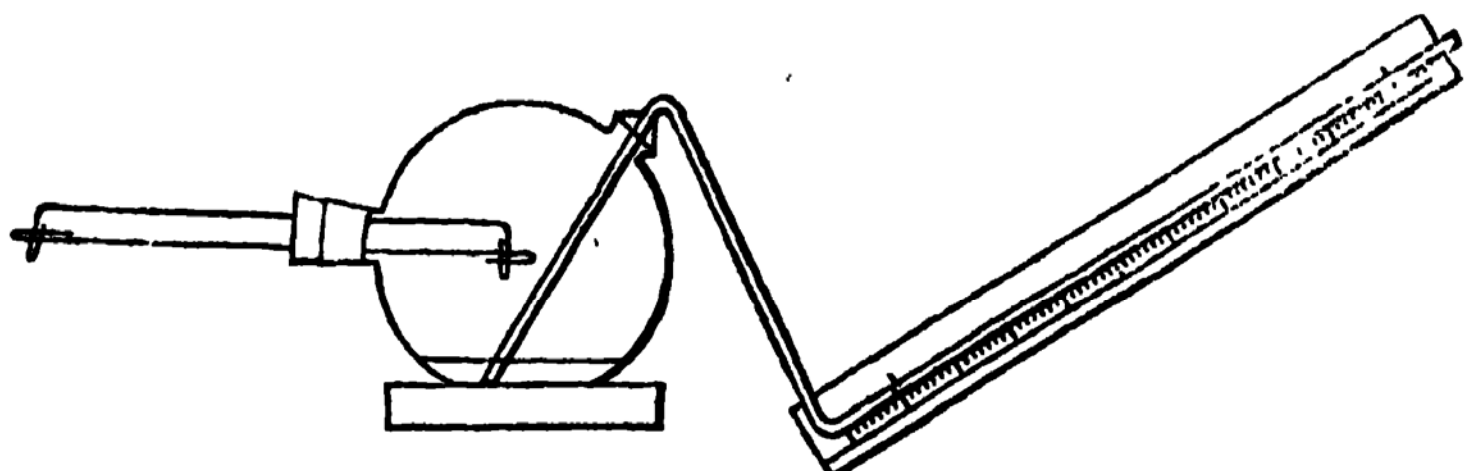
This cross turned over, on either of the slips, as an axis, or placed horizontally, vertically, or in any degree of obliquity, always exhibits the relation between the current and the needle: and being laid down, with its magnet corresponding with the position of the needle, it shows in what direction the current is passing over it; or turning it over upon the magnet as an axis, the reverse current is seen returning under it.

2. If there be any modification of electricity answering to that of Dr. Hare, a compound of electricity and caloric, this, from the mode of its generation, should be the one. But the same pair, of antimony and bismuth (1.), heated until they would drive the needle nearly round the circle, by the first impression, had not any appreciable effect on Mr. Harris's thermotest, though tried upon wires of various dimensions and different conducting powers. Thermo-electricity, then, seems to be no way distinguished as containing caloric.

Attractions and repulsions, or chemical decompositions would be precluded by its low tension; and these are its only apparent distinctions from the voltaic current, or from the electricity of the machine.

II. *Is Thermo-electricity produced at the expense of Caloric?*

3. Into the smaller neck of a tubulated receiver was fixed an open glass tube, reaching obliquely to the bottom, and bent, siphonwise, on the outside, so as to descend a little lower than the inner end: it was then again bent upward, nearly at a right angle, and to this rising end a scale was attached.



Into the receiver was poured water tinged with cochineal, and covered with a stratum of oil, to prevent evaporation by the heat. Suction being now applied to the end, the liquid remained, on the principle of the siphon, a few degrees up on the scale. A cork thoroughly soaked in grease being fitted to the open neck, the whole constituted a very sensitive air thermometer.

The cork was now perforated by two wires; one of copper, the other of tinned iron; the latter bent off at each end, so that it could be brought into contact with, or separated from the copper by a slight turn with the fingers. Both ends of each wire were doubled, flattened and polished to ensure good contact; they projected about three inches into the receiver, and nine inches outside.

Being now set in contact, the cork was drawn out, and the ends of the wires which entered the bulb were placed between a fold of sheet copper, and heated by holding them for a given time (25^s) over an Argand gas light, being turned over at 15°, to equalize the heat in both wires. The sheet copper being now drawn off, the hot ends were thrust into the bulb, and the cork pressed in air-tight. The fluid rose quickly to about 90° (more or less, depending on slight differences in the heat, and in the quickness and force with which the cork was pressed in). After a few seconds, the liquid began again to descend, by the cooling of the wires. They were then separated, when the descent of the fluid was suddenly checked: on renewing the contact, it fell with renewed impetus; and so stopped and resumed its fall at every separation and renewal of the contact.

Other wires of the same and different metals were tried, and with many different corks; but the effect was always con-

sistent, and indicated that the thermo-electric current did consume caloric in its production.

4. If this inference were true, it should follow that the wires heated to the same degree, should cool much more rapidly when kept in contact the whole time of cooling, than when kept all the time separate. They were therefore heated as before, and being thrust in, the fluid was allowed after rising to recede to 70° ; from which its time of descent to 20° , i. e. 50° *, was observed by a seconds watch,—the wires remaining in contact.

The same proceeding was repeated, the wires remaining separate.

Precision, for reasons above given (3.), was unattainable; and the conclusion was only to be drawn from the average of many experiments. This average gave 60° for the closed wires, and less than 60° for the separated ones; thus the cooling seemed retarded, rather than accelerated, by the contact, and consequent electrical current. All that repetition and examination could do to avoid discrepancy was attended to, and yet these experiments seemed to contradict the preceding ones.

5. The mode of interrogation was reversed. If caloric disappeared in the production of thermo-electricity, it was not unlikely that where caloric became latent, thermo-electricity might appear.

A glass siphon being suspended by the ends over a spirit lamp, tin was dropped in and melted there, until the siphon was nearly full; the ends were then connected with the poles of the magnetest by long and similar copper wires, tinned at the points. Although copper is (generally) positive to tin, no current ensued, because both ends were alike in contact with copper. A hot iron wire being now plunged into the tin, at one end of the siphon, gave a deflection of 6° ; and when transferred to the other end, a deviation to the same amount, in the contrary direction; and so repeatedly (37.): thus manifesting a current when electricity was developed. A slip of tin was now plunged into one end: of course no deviation could be expected (37.). This tin was allowed to melt there, by which caloric must have become latent. The fusion was gradual, and in full contact with metal on all sides; the electricity, if any were produced, must therefore have been continuous, and could not have acquired tension. It would

* Not degrees of Fahrenheit, or of any other standard scale; for the heat of the air about the wires must have been very different from that about the glass.

also have taken the copper, in preference to the tin, and the needle would have deviated accordingly.

But no deviation occurred.

Mercury, which is pretty near tin in thermo-electric power, was then substituted; and being heated to the melting point of tin, a warm iron wire was plunged in, which gave a deviation of 5° : but iron not coming well into contact with mercury, a tinned iron wire was tried, by which a deviation of 6° was obtained as before (5.). In this slips of tin were successively melted; but without any manifestation whatever on the needle.

Here we have no evidence of thermo-electricity, on the disappearance of caloric; in the preceding experiments we had no evidence of the disappearance or abstraction of caloric, in the production of thermo-electricity. To what, then, are we to attribute the contrary evidence of the previous experiments, as the apparatus was certainly air-tight?

6. When the wires were separated, the whole heated surface was exposed to the air in the bulb; and the portion between the heated ends would be most expanded, and least affected by the cooling influence of the glass. When they were in contact, this, the most effective part of the heated surface, was covered up. Hence a momentary expansion upon opening, and contraction upon closing the contact of the wires.

But since when thus in contact they cool less quickly, in proportion as they have less effect upon the air; so the whole time of descent, for 50° of the scale, should be greater, rather than less, when closed than when open.

If this were the true explanation, homogeneous wires should answer as well as those of different metals. Accordingly, two copper wires were passed through a cork, bent and heated as before (3.); and the descent of the fluid was found to be suddenly stopped and resumed, at the instants of opening and closing the contact, just as when the wires were of two different metals.

7. The evidence is therefore opposed to the expenditure of caloric in the production of thermo-electricity, and to the converse. Yet I thought the opposing experiments worth stating, (as they were not unlikely to occur to others,) to show that they had been tried, and shown to be inconclusive.

III. *How far is the Radiation of Heat, or any collateral property, concerned in the Development of Thermo-electricity?*

8. The result of M. Becquerel's exact researches, above quoted from the *Ann. de Chim. et de Phys.*, Aug. 1829, is, that the only known property of heat in which the metals take

the same order as in thermo-electricity, is that of radiation; and therefore he supposes that two pieces of hot metal, in contact, radiate in the same proportions as they would if separate and exposed; and that thermo-electricity is a result of this inter-radiation.

This is certainly applying to the term radiation a new meaning, very little analogous to its ordinary acceptance*. Yet the same quality, whatever it may be, which occasions radiation to be more or less copious, may also determine a superficial communication, proportionately copious, though different in mode. We have evidence, too, that the superficial absorbent power for heat, is proportionate to the radiating power; and it seemed not very improbable that here might be found some trace towards the solution of the problem.

9. If the effects were due to any cause connected with, or related to radiation, they should be modified by alterations in the surfaces; and by making the surfaces of a thermo-electric pair identical, their reaction should be greatly weakened, if not neutralized altogether.

An iron and a copper wire, of the same size ($10 \times \frac{1}{16}$ inch), were well tinned from end to end, and twisted together for an inch. A similar pair, untinned, but polished, were similarly joined; and the feet tinned for $\frac{1}{2}$ an inch, that the contacts with the mercury might be alike. Instead of the tinned wires having their efficacy impaired, they gave greater deflections than the clear ones. This unexpected fact, verified in a great number of ways, led to a long course of experiments; the general bearings of which will occupy a subsequent section (VIII.).

I am at a loss to reconcile with this fact any cause connected with radiation.

10. To try radiation more directly, and on a larger scale, a plate of tinned iron was wired round with tinned iron wire, which projected at one corner, for a conductor. A sheet of soft tin was also wired round with copper wire, projecting in the same manner, and for the same purpose. The sheet tin being laid on the tin plate, with a leaf of bank post paper between, to prevent contact, the conductors were plunged into the mercury of the magnetest, and a couple of lamps placed under the tin plate. The whole soon became hot, without deflecting the needle. The tin sheet was now lifted off, the paper removed, and a few threads spread about the

* [It appears to us, on the contrary, that M. Becquerel here uses the term radiation in a sense perfectly similar to its ordinary acceptance: see Sir J. Herschel's remarks on the process by which radiation and conduction slide into each other, in his *Prel. Disc. on Nat. Phil.*—*EDIT.*]

tin plate, to keep the sheet tin from touching it, whilst their surfaces should be fully exposed for radiation, at an extremely small distance. No deflection ensued on thus replacing the sheet tin; but on putting a slip of tin-foil in contact with both, the needle started off 25° .

A plate of polished copper was similarly used, first with sheet tin, afterwards with the tin plate; but with no effect whatever, in either case, until metallic contact was made.

The total inefficacy of such large radiating surfaces, added to the result of the preceding experiments, discourages the hope of tracing the cause of thermo-electricity to any superficial action analogous to radiation; and more convincing evidence to the same effect will appear (20. *et seq.*).

In fact I have been unable to find any table of radiation corresponding with Becquerel's thermo-electric order.

IV. *Is the proximate cause of Thermo-electricity in any way connected with Conduction?*

12. Having previously found reason, from the experiments of others as well as my own, to conclude that the heat produced by electricity in metallic conductors (and, with due allowance, in liquids and air,) is in the order of, and probably in proportion to the resistance it encounters in the body heated, whether that resistance was owing to inferior conducting property or diminished thickness, I was led by the following experiment, compared with some of Becquerel's, to suspect a reciprocity of action, such that electricity, restricted in its passage, producing heat; so heat, allowed to flow more freely, might produce electricity.

The circuit from a voltaic coil was divided into 5 parts, the two ends next the coil, and the 5th or middle piece, being of stout copper wire, the ends each 2 feet, the middle piece 4 feet long; the connexions from each end to the middle piece were fine silver wire $\frac{1}{100}$ th of an inch thick. Thus no electricity, positive or negative, could reach the middle piece except through these fine wires.

The charge being now made such as to warm the silver wires, a magnetic needle was moved along the circuit, which was laid (aportion of the middle piece, of course, excepted,) in the magnetic meridian. Its equidistance from the conducting wire was ensured, by the card, on which it was supported, being kept always in contact therewith. When over the copper, either of the middle or end pieces, the divergence was 36° ; when over either of the silver wires, 26° . Thus heat seemed to be produced, by restriction of the current, on entering the

silver wire; and the full electrical effect appeared to be restored when the obstruction was overpassed.

13. Becquerel found (*ubi supra*) that a circle of platinum wire, of equal diameter throughout, produced no current when heated in any part; but a knob being made in the circle, and heat applied near it, on either side, a current set always from the heated point towards the knob, where the heat had room to diffuse itself.

Also, that on soldering together the ends of his magnetest wire, no current ensued, when the point of junction was heated even to redness; but on touching the wire on either side of the heated point with a cold piece of the same metal, the current immediately set towards the part so touched.

14. It is true that the tables of conduction for heat and electricity, contrasted by Cumming with that of thermo-electricity, deny the presumption of any connexion between them. But there would appear to be two properties of conduction for caloric,—promptitude and final efficacy. For although we handle platinum wire, at two inches distance from a red-hot point, in operations with the blowpipe, which hardly any other metal will admit of, yet M. Despretz found that a rod of platinum *eventually* became hotter, at a given distance from a given source of heat, than any other metallic rod of equal dimensions; and if thermo-electricity be influenced by conduction at all, it must be by promptitude of conduction, for very short distances: whilst the tables hitherto published refer to a property compounded of the two, or intermediate between them.

A copper and an iron wire, drawn so that equal lengths corresponded to the atomic weights, were bound with thread for an inch each, cut off at $\frac{1}{2}$ an inch above the binding,—this $\frac{1}{2}$ -inch bent over in the form of a hook, and the end filed flat. The two wires, thus prepared exactly alike, were fixed to a little wire frame, to keep them parallel and separate, and the filed ends dipped into melted lard. A couple of orange peas, of equal weight and size, were also dipped in the lard. When cold, the cohesion of the lard was sufficient to support the little balls against the ends of the wires. The bound part of the wires was now plunged into warm mercury, and the ball soon fell from the *copper* by fusion of the lard. The wires were then cooled by plunging the bound part into cold mercury, and the balls interchanged. On replunging into warm mercury, the ball fell again from the copper; and this always took place on repeated interchange of the balls, and alternation of cold and heat; the ball falling every time from the copper, until the mercury was cooled so far that the lard would not melt, and so that neither ball would fall. This was decisive

in favour of the conducting power of copper, down to a very low heat, and at $\frac{1}{2}$ an inch distance.

15. To try at still shorter distances, two other lengths of the same wire were twisted together for $\frac{1}{2}$ an inch, then separated for an inch, and the other ends brought near together, for holding in a pair of cold pliers. (The twist was tarnished, to prevent metallic contact between the mercury and copper, which also was the object of the thread-binding in the former part of the experiment.) Spots of lard were now placed, at equal distances from the twist, on the iron and on the copper, and the twist plunged into the warm mercury. The lard became transparent (by melting) first on the copper: and this on repetition, whatever the distances, down to $\frac{1}{8}$ th of an inch; and whatever the temperature, so long as it was sufficient to melt the lard at all. Varying the sizes and proportions of the wires, tinning their surfaces, to prevent unequal waste of heat, *in transitu*, by radiation, did not alter the result. In every instance, for equal distances, the lard first became transparent upon the copper wire; which is thereby proved, for low heats and short distances, to retain its character as the better conductor for caloric.

16. To ascertain the conducting powers for electricity of very low tension, a pair of iron and a pair of copper wires, atomically proportioned as before, and each 2 inches long, had $\frac{1}{2}$ an inch at each end bent down at right angles, and the points tinned, for equal contact with the mercury. Through these wires, two other mercury boxes were connected with those of the magnetest, an iron and a copper wire being employed on each side. Into the further boxes dipped the ends of a thermo-electric pair, of copper and tinned iron, of which the other ends were twisted together, and the twist was exposed to an equable heat, until the needle became stationary at 18° .

The short iron wires were now removed, leaving the conduction to be carried on by the short copper ones only: no appreciable change took place in the deflection.

The iron wires being now replaced, the copper ones were removed, leaving the conduction entirely to the iron. The needle receded more than a degree, and sometimes nearly 2° , confirming the superiority of copper as an electrical conductor, for extreme low tension and very short distances.

17. But as heat also enters into the question, the apparatus remaining as described, a spirit-lamp was brought under the middle of the copper leg of the thermo-electric pair, and continued there until the point acted upon by the flame became red hot. Little or no change took place in the deflection.

The lamp being then removed to the iron leg, the needle receded 2° before the heat had attained redness. Thus the conducting power of iron suffered more from heat than that of copper.

18. Iron being thus proved to be inferior in conducting power, both for heat and electricity, in circumstances directly in point, to copper, cannot owe its thermo-electric superiority to that property; and, extending the analogy to other metals, the inference at the beginning of this section would seem to be refuted. But the following section will bring the question before us in a different point of view.

[To be continued.]

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 15. **A** PAPER was read, entitled, "On Improvements in the Instruments and Methods employed in determining the Direction and Intensity of Terrestrial Magnetism." By Samuel Hunter Christie, Esq. M.A. F.R.S.

The tedious nature of the observations by which the direction and intensity of the terrestrial magnetic force are determined, and the uncertainty attending the results when obtained, have long been a subject of regret to all who are engaged in the investigation of the phenomena of terrestrial magnetism. Sensible of this, the author's attention has at different times been turned to the improvement of the instruments employed for these purposes; and in this communication he proposes methods by which he considers that these instruments might be so improved that the results should be obtained with greater facility and also with greater certainty. The uncertainty attending the results obtained with the dipping needle, as at present constructed, arises principally from the two sources, friction upon the axis, and the want of coincidence of the needle's centre of gravity with the axis of motion; the latter rendering necessary the inversion of its poles. The author suggests a method by which he considers that, probably, the friction may be diminished; but he has principally directed his attention to obviate the necessity of the inversion of the poles.

In order to remove the practical difficulty attending the adjustment of the centre of gravity to the axis of motion, an operation in which the artist rarely, if ever, completely succeeds, the author proposes to dispense with this condition; and shows how the dip may then be determined, without the necessity of inverting the poles of the needle, the position of its centre of gravity having been determined previously to its being magnetized. The advantages attending the method proposed by the author are not, however, restricted to the determination of the dip with greater accuracy and greater facility: a further and still greater advantage attending the use of a dipping needle on the principle he proposes, is, that a measure of the terrestrial magnetic intensity will be obtained by the same observations which give the

dip ; so that, by this means, the observations usually required for that purpose, and which are of the most tedious nature, will be avoided. To effect both these objects in the most convenient manner, he proposes that the needle should be so constructed that its centre of gravity should be out of the axis of motion, in a line perpendicular to that axis and to the axis of the needle. The requisite formulæ for determining the dip and the measure of the terrestrial intensity, in this case and also when the centre of gravity is in any other position, are investigated in the paper. Mayer had previously pointed out that the dip might be determined by means of a needle having its centre of gravity out of the axis of motion, and had given the formulæ requisite for that purpose. His object, however, does not appear to have been the same as our author's,—the avoiding in all cases that source of inaccuracy, the inversion of the poles of the needle,—but simply the determination of the dip, whether the centre of gravity of the needle were made to coincide with the centre of motion, or not : the determination of a measure of the terrestrial intensity, by such means, does not appear to have entered into his contemplation.

As another form in which the same principles might be advantageously applied, the author proposes that two needles, similar in all respects, should be placed on the same axis ; and points out how, by means of such a compound needle, both the dip and intensity might be determined by independent methods, so that the agreement of the results would afford a test of the accuracy of the adjustments and of the observations. He considers that the knife-edge support, which has recently been adapted to a dipping needle, would be peculiarly applicable to a needle of this construction. The sensibility of such a needle would be much greater than that of any hitherto constructed, and the utmost delicacy would be required in the adjustments ; but if the needle were accurately constructed, and due care were taken in the magnetizing, and in making the adjustments and observations, the author expects that the dip and intensity would be determined to a degree of certainty hitherto unattained.

The advantages proposed to be derived from the use of a dipping needle on the principle described in this paper, are, that as the dip would be obtained without inversion of the poles, the results would be less liable to error than when that operation is necessary, and the observations would be made in less than half the time usually required ; and that a measure of the intensity of terrestrial magnetism would be obtained from the same observations which give the dip, the intensity of the force being thus always determined by means of the same needle, and at the same instant as its direction.

May 2.—A paper was read, entitled, “ Essay towards a first approximation to a Map of Cotidal Lines.” By the Rev. William Whewell, M.A.F.R.S. Fellow of Trinity College Cambridge.

The general explanation of the phænomena of the tides originally given by Newton, although assented to by all subsequent philosophers, has never been pursued in all the details of which its results are susceptible, so as to show its bearing on the more special and local phænomena, to connect the actual tides of all the different parts

of the world, and to account for their varieties and seeming anomalies. The first scientific attempt that was made to compare the developed theory with any extensive range of observations, was that of Daniel Bernouilli in 1740: the subject has since been pursued by Laplace and Bouvard, and still more recently by Mr. Lubbock. But the comparison of contemporaneous tides has hitherto been unaccountably neglected: and to this particular branch of the subject the researches of the author are in this paper especially directed; the principal object of his inquiry being to ascertain the position of what may be called *cotidal lines*, that is, lines drawn through all the adjacent parts of the ocean where it is high water at the same time; as, for instance, at a particular hour on a given day. These lines may be considered as representing the summit or ridge of the tide-wave existing at that time, and which advances progressively along the sea, bringing high water to every place where it passes. Hence the cotidal lines for successive hours represent the successive positions of the summit of the tide-wave, which in the open sea travels round the earth once in twenty-four hours, accompanied by another at twelve hours' distance from it, and both sending branches into the narrower seas. Thus a map of cotidal lines may be constructed, at once exhibiting to the eye the manner and the velocity of all these motions.

Although the observations on the periods of the tides at different places on the coast and different parts of the ocean, which have been at various times recorded, are exceedingly numerous, yet they are unfortunately for the most part too deficient in point of accuracy, or possess too little uniformity of connexion to afford very satisfactory results, or to admit of any extended comparison with theory. With a view to arrive at more correct conclusions, the author begins his inquiry by endeavouring to determine what may be expected to be the forms of the cotidal lines, as deduced from the laws which regulate the motions of water: and he proceeds afterwards to examine what are their real forms, as shown by the comparison of all the tide observations which we at present possess.

The paper is divided into five sections. In the first the author treats of cotidal lines as deduced theoretically from the known laws of the motion of fluids. On the supposition that the whole surface of the globe is covered with water, the cotidal lines would coincide with the meridians, and would revolve round the earth from east to west in something more than twenty-four hours, with a velocity of nearly 1000 miles an hour at the equator. The form and the regularity of these lines would be materially affected by the interposition of land in different parts of this ocean, whether in detached islands, or groups of islands, or large continents, occupying a considerable portion of the surface. In these cases the primary wave will be broken, deflected and variously modified, so as to give rise to secondary or derivative tides, sometimes separating into branches, and producing points of divergence; sometimes uniting at various places, or points of convergence; and at other times producing, by more complex combinations, various phenomena of interference, and other apparently anomalous results. Such is the general character of the tide-

waves that actually proceed along the coasts of the Atlantic : and the modifications in their course and velocity are still more perceptible in bays, gulfs, and narrower channels and inlets of the sea, as well as in their progress along rivers. The author traces in detail the effects which these different circumstances may be expected to produce. He adverts to an important distinction which has frequently been lost sight of, between the progressive motion of the tide-wave and the actual horizontal motion of the water, or tide-current ; motions which do not bear any constant relation to one another. Hence the change in the direction of the current does not invariably indicate the rise or fall of the water.

In the second section he examines the causes which have led to inaccuracy in making observations on the tides ; the first of which is dependent on the circumstance just mentioned, of the occasional want of correspondence between the times of *high* and of *slack* water ; the former referring to the moment of greatest elevation, the latter to that when the direction of the current changes. The other causes of error are derived from the change which takes place in the course of the day in the moon's angular distance from the sun ; from the half-monthly inequality in the establishment, arising from the relative position of the sun and moon during each lunation ; and from the necessity that exists of making a correction for what may be termed the *age* of the tide ; that is, the interval of time which has elapsed between the period of the origin of the wave and the time of its actual arrival at the place of observation.

The third section, which forms the chief bulk of the paper, is occupied by a statement and discussion of the tide observations now extant, and which the author has, with great industry, collected from a variety of sources, both of published accounts, and of manuscript documents preserved in the Admiralty. Commencing with the tide-waves, first of the eastern and then of the western coasts of the Atlantic, he follows them to the Northern sea, and to the different coasts of the British islands, and of the German Ocean. He passes next to the examination of those of the Southern Atlantic at Cape Horn and the adjacent coasts ; thence tracing them, as far as the present imperfect data will allow, along the western shores of the American continent, to the central parts of the Pacific, and in their progress across the Australian and Indian Oceans. He likewise examines the condition of the tides in rivers, as to the magnitude and velocity of the undulations, the occasional production of a high and abrupt wave, or *bore*, and as to the influence of the natural stream of the river upon the different periods of elevation or depression of the water.

The fourth section contains general remarks on the course of the tides, suggested by the preceding review of the phenomena they present ; on the velocity of the tide-wave ; on the form of the cotidal lines ; on the currents which attend the tides ; on the production of revolving currents ; on the magnitude of tides ; and on the constancy of the cotidal lines. He adverts also to some peculiarities resulting from interference, such as the differences of the two diurnal tides, and occasionally the occurrence of single day tides.

In the concluding section the author offers various suggestions respecting the most eligible mode of making observations on the tides, and of correctly reducing them when made.

GEOLOGICAL SOCIETY.

February 27.—A paper, commenced during the last Session, entitled “Description of Parts of the Kingdoms of Valencia, Murcia, and Granada, in the South of Spain,” by Capt. Cook, R.N. F.G.S., was concluded.

A paper entitled “Observations relative to the Structure and Origin of the Diamond,” by Sir David Brewster, K.C.H. LL.D. F.G.S. &c., was then read.

In the year 1820, the author communicated to the Royal Society of Edinburgh a singular fact relative to the structure of the diamond, accompanied with some conjectures respecting the origin of this remarkable gem:—the present essay may be viewed as a continuation and extension of the same inquiry.

The author refers to the remark of Newton, that amber and the diamond have a refractive power three times greater in respect of their densities than several other substances; and he quotes Newton's conjecture, founded on that remark, of the diamond being probably, like amber, an unctuous substance coagulated. In proof of the intimate relation between the inflammability and absolute refractive power of bodies, Sir D. Brewster adds the facts, that sulphur and phosphorus exceed even the diamond in absolute power of refraction, and that these three inflammables stand before all solid and fluid substances in their absolute action upon light.

Another close analogy between the diamond and amber, independently of their like locality and carbonaceous nature, was traced by the author in their polarizing structure. Both of these minerals contain within their substance small cells or cavities, filled with air, the expansive force of which has communicated a polarizing structure to the parts in immediate contact with the air. The description of this structure, which is displayed from sectors of polarized light encircling the globule of air, is illustrated by drawings.

The author contends that the peculiar polarizing power around the cavities in amber and in the diamond must have been occasioned by the expansive force of the confined substance, supposed to be gaseous, compressing the sides of the cells, while the substance of the minerals was in a soft and yielding condition. A similar structure may be produced in glass, or in gelatinous masses, by a compressing force, propagated circularly from a point.

Having thus shown that the diamond was at one time in a soft or pasty state, the author argues that this state was not produced by igneous fusion. For in his laborious examination of the cavities in crystals, both natural and artificial, such as topaz, quartz, amethyst, chrysoberyl, &c., and in salts, he observed the condition of many thousand cavities; but in no case, neither in crystals formed by means of igneous fusion nor by aqueous solution, did he observe

a single cavity in which the expansible fluid within had communicated a polarizing structure, similar to that around the cavities in the diamond. He believes, therefore, that the softness must have been that of semi-indurated gum; and that the diamond was derived from the decomposition of vegetable matter, as is admitted to have been the case with amber. The crystallized condition of the diamond is not to be considered as decisive against this inference, since the mineral called mellite has a distinct crystallized form, while its composition and locality attest a vegetable origin*.

A notice "On the Occurrence of the Bones of Animals in a Coal-mine in Styria," by Professor Anker, of Joanneum in Gratz, was then read.

March 13.—A paper, entitled "Geology of the Environs of Bonn," by Leonard Horner, Esq. F.G.S. F.R.S. &c., was read.

The district described by the author lies on both sides of the Rhine,—the Siebengebirge, or Seven Mountains, constitute the chief feature; and the highest point in the group, the Oelberg, is 1369 English feet above the level of the sea. It possesses peculiar interest to the geologists of England, as being the nearest point where volcanic phænomena, approaching in character to those of modern times, can be seen.

The lowest stratified rock is grauwacke, which seems to belong

* [As the subject of the origin of the diamond was discussed in our pages some years ago, we annex an historical remark or two to the above abstract of Sir D. Brewster's paper. In the communication to the Royal Society of Edinburgh, alluded to in the abstract, it was argued that the original compressible state of the diamond could not have arisen from the action of heat, on account of "the nature and recent formation of the soil in which it is found." Mr. Brayley, when discussing the arguments in favour, respectively, of the vegetable and the mineral origin of this gem (in a notice headed "*Origin of the Diamond*," published in the *Phil. Mag. and Annals*, N.S., vol. i. p. 147—149, and signed E. W. B.), observed, that the discovery, subsequently to Sir D. Brewster's researches, of a diamond in a matrix of brown ironstone occurring in beds subordinate to chlorite slate in the Brazils, and the information, also subsequently promulgated, that the diamonds of Southern India are found in an ancient breccia, removed Sir David's objection on this point. Mr. Brayley inferred, therefore, since, for aught that had been advanced to the contrary, the compressible state once possessed by the diamond might have resulted from the action of heat, that the optical characters discovered by Sir D. Brewster, did not necessarily refer its origin to the vegetable kingdom.

In the paper read before the Geological Society, however, Sir D. Brewster shows, in a direct manner, by comparing the optical characters of the diamond with those of other crystallized substances, that the original soft or pasty state of the former could not have been produced either by igneous fusion or by aqueous solution. The evidence now adduced on this point is therefore quite independent of the geological situation of the gem; and it would appear to be impossible to refer it to any other than a vegetable origin, as originally suggested by the author of the paper.

We observe that Dr. Thomson's statement of the geological situation of the diamond (*Inorg. Chem.* vol. i. p. 156.) is derived from the above-cited notice in the *Phil. Mag. and Annals*.—EDIT.]

to the later ages of that deposit, and to approach in character, in some parts of the district, to the old red sandstone. There are no associated beds of limestone. The strata are in general highly inclined, but they are found at all angles; and there is neither uniformity in the strike nor in the dip; the strike is most usually N.E. and S.W., the dip more frequently S. than N. In the immediate neighbourhood of the Siebengebirge, the strata are thrown up in all directions, evidently by the eruption of the volcanic matter.

The whole of the later secondary series is wanting, and the grauwacke strata are covered, unconformably, by a tertiary deposit, consisting of beds of sand, sandstone, clays, and lignites, which collectively constitute a brown coal formation. This is covered by an extensive bed of gravel, and above the gravel is a loosely coherent, sandy loam, containing land shells of existing species, and called in the Rhine valley, *Loess*. From under the grauwacke there have burst forth a variety of unstratified rocks, consisting of various modifications of trachyte, trachytic tuff, basalt, and other modifications of trap. The main body of the Siebengebirge is composed of these volcanic rocks.

There are many varieties of the trachyte, from a highly crystalline rock, with separate crystals of felspar of great size, very like a large-grained granite, to a compact stone of uniform structure, like compact felspar or phonolite. The trachytic tuff also assumes various appearances, from that of a coarse conglomerate to a white earthy substance, scarcely distinguishable from chalk. There is no evidence of the trachyte having flowed in a stream, and the author saw it only in one place in the form of a dyke. There are several varieties of trap, but the most common is a compact black basalt, in many places in perfect columns. There are numerous dykes of basalt. A remarkable eruption of trap tuff, penetrated by basaltic dykes, occurs at Siegburg, where three cones, of about 200 feet in height, rise abruptly in the midst of an alluvial plain, nearly on a level with the Rhine.

The author points out the affinity which Von Buch has shown to exist between the mineral composition of all the unstratified rocks, and how a series of insensible gradations could be formed, through trachyte and the trap family, from granite to modern lava. He shows that a suite of specimens could be collected in the Siebengebirge, passing insensibly from large-grained white trachyte to compact black basalt; and that these hills afford many interesting facts corroborative of the opinion advanced by M. Gustave Rose respecting the identity of hornblende and augite. Notwithstanding, however, this connexion between the several volcanic rocks, the author points out distinct evidence of different epochs of formation among them. He is of opinion that the greater part of the trachytic tuff was the first ejected; that it was similar to those showers of scorïæ and ashes which frequently precede the eruptions of streams of lava; and that it is not, as some previous writers have supposed, a rock recomposed from the disintegration of pre-existing trachyte. He saw the trachytic tuff traversed in one place by a dyke of trachyte, and it contains numerous

balls, like volcanic bombs, of varieties of trachyte, quite distinct in character from any found *en masse*. It is traversed in many places by trap dykes; and as these last are also found traversing solid trachyte, the subsequent eruption of the trap is demonstrated. He discovered no instance of the recurrence of trachyte after basalt had begun to flow. There is on the side of the Rhine, opposite to the Siebengebirge, an extinct volcano, of comparatively modern date, the Rodderberg, composed of cinders and scorified rocks. The crater is about a quarter of a mile in diameter, and a hundred feet deep: a farm-house, surrounded by corn-fields, stands in the middle of it. #

The brown coal formation is composed of beds of loose sand, sandstone, and compact siliceous conglomerate, which often, in mineral structure, cannot be distinguished from many varieties of *grauwacke*; of clay, abounding with balls and layers of clay ironstone; and of many varieties of lignite, from the state of a light brown earth, to a black compact shining mass, or jet. All of these are frequently met with in thick beds, and the lignite is most extensively worked for fuel. They contain numerous impressions of leaves, and stems of trees are very abundant. With the exception of casts of *Lymnæa* and *Planorbis*, in an opaque white chert, of very limited extent, no shells, fresh-water or marine, nor any remains of mammalia or birds, have been found in any part of the formation; but in some beds of the lignite, impressions of fresh-water fishes, the *Leuciscus papyraceus* of Agassiz, are found in great abundance, and there have also been found extinct species of frog, salamander, and triton, together with remains of insects, which Professor Goldfuss considers to belong to the genera *Lucanus*, *Cerambyx*, *Anthrax*, *Cantharis*, and eight others.

The author submitted the specimens which contain impressions of leaves, to the examination of Professor Lindley. Most of them are in too imperfect a state to admit of any accurate determination, but they consist generally of casts of portions of dicotyledonous leaves; and among them are two species, the existence of which is sufficient to determine the relative age of the formation, and, with great probability, the then warm climate of the North of Germany, viz. *Cinnamomum dulce* and *Podocarpus macrophyllus*: there are besides impressions of leaves very clearly belonging to the Palm. It is remarkable, however, that a recent examination by Professor Nöggerath of Bonn, and M. Cotta of Heidelberg, of an extensive suite of the woods found in this brown coal formation, did not disclose a single instance of a monocotyledonous tree.

A vast deposit of gravel, chiefly composed of quartz, but containing also a few fragments of basalt, trachyte, transition limestone, and *bunter sandstein*, lies over the brown coal formation, sometimes being only a thin covering, at others attaining a thickness of 125 feet. It is very distinct in character from the gravel now forming the bed of the Rhine, and is older than some of the volcanic eruptions, for a patch of it rests on the edge of the crater of the Rodderberg, covered by volcanic ashes.

The author next proceeds to point out what he considers the rela-

tive age of this brown coal formation, a task extremely difficult, from the almost total absence of shells, and the imperfect state of the means of determining an epoch of formation by fossil plants. By previous writers it has been assigned to the plastic clay of the Paris basin; but it appears to the author to possess no other character of identity than the mineral composition of some of the beds, and the occurrence of lignite, which prove nothing as to age. The amphibious animal remains resemble those of the great fresh-water deposit of *œningen*; but the few shells which occur, and the plants, are identical in species with many of those occurring in the older fresh-water beds of *Alx* in Provence. It seems to be very clear, that it is an exclusively lacustrine deposit, and the organic remains, the only evidence of age to be relied on where there is none from superposition of other beds, imperfect as they are, would seem to indicate a more modern date than the plastic clay. The author states that a distinguished geologist of Bonn has expressed his belief that it is even older than the chalk; but that although the opinions of that experienced observer are entitled to great respect, he cannot reconcile the phenomena described with anything known respecting the secondary rocks.

The determination of the age of this brown coal formation is of the highest importance, as fixing the periods of eruption of the extinct volcanos of the Lower Rhine; for the author of this paper shows, that the trachytic tuff contains leaves of plants identical with those found in the clay and sandstone deposits; that extensive layers of trachytic tuff are interstratified with the beds of the formation in many places; and that in one situation a mass of basalt, thirty feet thick, lies upon beds of coal thirteen feet in thickness. The conclusions which the facts appear to the author to justify, are, that there existed a vast fresh-water lake, in which the brown coal beds were deposited; that *during* that deposition volcanos burst forth at the bottom of the lake, as they do now at the bottom of the sea; and that a continuance of volcanic action, or of elevatory force, raised the Siebengebirge after the deposit had ceased,—perhaps at the very time when the basalt or trap eruptions took place, since near the summit of the Mandenberg, a columnar basaltic cone, there is a patch of brown coal beds at the height of nine hundred feet above the surface of the Rhine.

The last great formation, if it may be so termed, of this district, lying upon the gravel in which the present bed of the Rhine is cut, is that most remarkable deposit the *Loess*, a friable sandy loam, full of existing species of land shells, without river shells, and without plants, but containing bones of the *Elephas primigenius* and *Rhinoceros tichorinus*. It is found in detached masses, of vast thickness; but without any signs of stratification, and sometimes at a height of 600 feet above the Rhine, and may be traced with scarcely any interruption from Bonn to Basle, a distance of 250 miles. The author states that all the facts yet observed with respect to it, lead him to conjecture that its origin may have been owing to the sudden bursting of an extensive lake situated somewhere between Constance and Basle, and that subsequent denuding causes have carried away the

enormous mass of matter deposited by this sea of mud, leaving only detached portions as monuments of the passing of the mighty torrent.

March 27.—The first part of a memoir “On the sedimentary deposits which occupy the western parts of Shropshire and Herefordshire, and are prolonged from N.E. to S.W., through Radnor, Brecknock, and Caermarthenshires, with descriptions of the accompanying rocks of intrusive or igneous characters,” by Roderick Impey Murchison, Esq. F.G.S. F.R.S. &c., was read.

After adverting to the want of definite knowledge of the order of succession, and of the fossiliferous characters of those great deposits anterior to the old red sandstone, and commonly called transition rocks (fossiliferous grauwacke, De la Beche), the author states that the task of attempting to separate them into distinct formations, was first suggested to him by the very clear and perfect exhibition of their details in the country under review.

The present work has already occupied large portions of the two last summers, and has for its basis the maps of the Ordnance survey, coloured geologically by the author. To the Master General of the Ordnance, to Col. Colby, Capt. Robe, and the Officers of His Majesty's Map-office, for their assistance in supplying him with good geographical data, and also to Mr. Budgin, one of the Field-Surveyors of the Ordnance, the author acknowledges his obligations. He next adverts to the unpublished yet valuable observations of Mr. Arthur Aikin, made many years ago in the north-eastern portion of the country described; and he further expresses his sincere thanks to many resident gentlemen who assisted him in his observations.

The memoir is divided into three parts. The first contains an account of the overlying deposits of new red sandstone, coal measures, mountain limestone, and old red sandstone, each of which is in contact with, or contiguous to, the transition rocks (grauwacke series) in some portion of the region described. The second and most extensive part explains the subdivisions and relations of the grauwacke series as exhibited within a zone of country extending from the Wrekin near Shrewsbury on the N.E., to the mouth of the Towy, near Caermarthen, on the S.W.; and the third part is to be devoted chiefly to the consideration of the rocks of intrusive or igneous characters, and their effects upon the associated strata.

Part I. On the New Red Sandstone, Coal Measures, Mountain Limestone, and Old Red Sandstone.

1. *New red sandstone*.—This is the youngest secondary formation in contact with the transition rocks, and is exhibited on both sides of the Severn, near Shrewsbury, being superposed to coal measures, to various members of the grauwacke series, and to trap rocks of different characters, in all of which situations it is undisturbed.

The oldest strata of this formation are compared with the *rothe-todte-liegende* of Germany, or the older new red of the North of England, and are shown to underlie a dolomitic conglomerate at Alberbury and Cardeston. The superior members on the north bank of the Severn, consist of fine-grained sandstones, for the most part red, but offering at Grinshill, 7 miles N.E. of Shrewsbury, a subordinate,

white building-stone, of excellent quality. Small quantities of the ores of copper and cobalt, are mentioned as occurring in the formation near Grinshill and Hawkstone, &c. Much sulphate of barytes and decomposed pyrites are diffused through the mass of these rocks. No organic remains have yet been discovered in them.

2. *Coal Measures.* a. *Coal field of Coalbrookdale.*—This coal-field is stated to rest at Steeraways and near Little Wenlock, on a thin band of limestone, which, from its fossils, is shown to be true mountain limestone; whilst in the contiguous extension of this field, the carboniferous strata overlie, *unconformably*, various members of the grauwacke series, with one of which, the transition limestone of Wenlock Edge, they are brought into *conformable* apposition at Lincoln Hill, on the Severn. The complicated relations of the deposits within this small and disturbed district, east of the Wrekin, are referred to the protrusion of basalt and green-stone, which occasionally tilt the strata at high angles, and sometimes occupy the seats of faults.

b. *Coal-fields in the immediate vicinity of Shrewsbury.*—Of these, the most important is a curvilinear zone extending from the north-eastern flank of the Brythin Hills to Wellbatch, near Shrewsbury, the carboniferous strata reposing on the inclined edges of the grauwacke rocks, and dipping towards a common centre beneath the new red sandstone. Detached portions of the same zone are again found at Sutton and at Uffington; and they also follow the sinuous outline of grauwacke on the northern flanks of the Longmynd and Caer Caradoc. At Pitchford, the whole carboniferous series is represented by a bituminous breccia of a few feet in thickness.

Three thin beds of coal are, for the most part, observable, and the deposit is distinguished by an included band of limestone, similar in mineral aspect to the lacustrine limestones of Central France, and containing minute shells referrible to *freshwater* genera. The vegetable remains of the associated shales are chiefly analogous to the plants of other coal-basins; but those of Le Botwood are rich in the new species, *Neuropteris cordata*, whilst the shale of Pontesbury has offered a beautiful example of *Pecopteris blechnoides* in fructification.

After demonstrating the slight commercial value which can be attached to the thin deposits of this age, the author speculates on the probable importance of the outer zone or Pontesbury-field, which he presumes may expand to a great thickness in its passage beneath the new red sandstone of N. Shropshire and Cheshire.

c. *Coal fields in the Clee Hills.*—These fields are thrown up to considerable heights above the adjoining country of old red sandstone, both in the Brown Clee, and the Titterstone Clee Hills, the coal being for the most part covered with basalt.

The Brown Clee is distinguished by two tabular summits of black basalt (Jewstone), the highest of which is 1806 feet above the sea. The coal-bearing strata have for their base a hard sandstone occasionally conglomeritic, the equivalent of the millstone grit. On three sides of this ridge, these very thin and poor coal measures repose on old red sandstone, which to the west is a coarse conglom-

merate; but on the fourth or south-eastern side, there is interposed between the old red and the lower coal grits, a thin zone of limestone, which the author, therefore, refers to the age of the mountain limestone. Several faults are mentioned as traversing this coal tract from S.W. to N.E., one of which has been the source of eruption of much basaltic matter.

The Titterstone Clee Hill is next described; and details are presented of those parts only which were unnoticed in Mr. R. Wright's memoir. The most important of these relate to the Knowlbury field, which, from its juxta-position to the larger field of Coalbrook, the author terms a parasitic basin, and shows that it contains five seams of coal and some bands of ironstone. The strata around the verge of this basin are highly inclined, the dip diminishing as it approaches towards a common centre. Sections across this small basin exhibit considerable faults, which always occur as upcasts towards the higher sides of the hills, where the basaltic matter has found vent: coal, included between two of these upcasts, is described as much thickened and in the state of cannel coal.

Many fossil plants of new species from the Knowlbury and Gutter Works are described by Professor Lindley. Recent investigations of Mr. Lewis are mentioned, which prove the existence of a central dyke or funnel of basalt, thereby confirming the opinion formerly expressed by Mr. Bakewell; and a complete transverse section of these hills shows, that some beds of coal have been carried up on the top of the basalt, and that this rock has flowed laterally so as also to overlie the coal. Although these coal measures rest, in the greater part of their circumference, on the old red sandstone, the existence of an interpolated band of true mountain limestone is pointed out, which from the thickness of a few feet near Bennett's-end, extends to a maximum development of about 60 feet beneath one part of the Cornbrook field, where it contains subordinate beds of fine oolite, marl of various colours, abundance of characteristic organic remains, and is much contorted and dislocated.

At Orelton, near the north-eastern extremity of this range, and thus connecting it with the more eastern coal-fields, is a tortuous range of oolitic mountain limestone, which rests upon the old red sandstone and, exhibiting some extraordinary fractures and dislocations, passes beneath the unproductive or lower coal grits.

3. *Old red sandstone*.—In the vast formation comprehended under the term old red sandstone, the author includes all the red or green marls, conglomerates, sandstones, limestones, or flagstones, the youngest or highest beds of which pass immediately beneath the mountain limestone or carboniferous strata, and the lowest overlie and graduate into, the superior members of the grauwacke series. The author gives a geographical sketch of the western side of the great trough in which this formation is deposited in Shropshire, Herefordshire, and Brecknockshire; the prevailing strike of which is shown to be from N.E. to S.W., and the usual inclination to be to the S.E. The upper beds of the formation, near the Brown Clee,

and partially near the Titterstone Cleve Hills, expose a thin band of conglomerate; then follow, in descending order, red or green marls, with two or more zones of impure limestone called cornstone. To these succeed micaceous flagstones and thin-bedded building stones, with other strata of marls and cornstone. Massive varieties of concretionary limestone, termed ball-stones, range along the western foot of the Brown Cleve. They are sometimes from 18 to 20 feet thick, and are very dissimilar in quality and in appearance from the thin and conglomeritic bands of the rock. Alternations of red and green marls again succeed beneath the cornstones, and the base of the whole formation is usually marked, particularly in its course from Kington to Caermarthenshire, by highly micaceous greenish and reddish tile stones associated with marl. Thick-bedded, fine-grained building stones of excellent quality are worked near Hay in Herefordshire, overlying the tile stone division. No workable seam of coal has ever been discovered in the old red sandstone. Dr. Lloyd has recently discovered near Leominster and Ludlow, in the central and calcareous sandstone beds of the formation, fossils which are chiefly referrible to undescribed species of the family of *Trilobites*, and with them a few fragments of plants apparently terrestrial.

An expansion upon a large scale is pointed out in the old red sandstone, which, from a narrow tongue, is described as extending all over the forest of Mynidd Eppint, on the western side of which it reposes conformably and at high angles, upon the uppermost strata of grauwacke, at the chief escarpment of that rock. Many transverse sections from the grauwacke formations to the edge of the Glamorganshire coal-basin are given by the author, and they exhibit a perfect conformability between the upper beds of the old red and the lower members of the mountain limestone, as well as a gradual passage from the old red into the grauwacke. He, however, insists that there are no two formations of the English series which can be better separated from each other for purposes of geological illustration, than the old red sandstone and the uppermost grauwacke; the former being as poor as the latter is rich in organic remains, whilst the colours and mineral characters of the two formations are also very distinct. The maximum thickness of the formation is not easily defined with accuracy, but the author has no hesitation in saying that it exceeds 4000 feet.

In the latitudes of Llandovery and Llandilo, the whole formation is thrown so much on edge, that it necessarily occupies a very small superficial breadth, whilst the very slight inclination and the undulation of the beds in Herefordshire and Brecknockshire account for its vast expansion in these counties.

Detached tracts covered with this formation are pointed out as occurring far within the frontier of the grauwacke rocks; and they are considered to be true basins of elevation which have been formed on the western sides of certain anticlinal lines, along which the inferior sediments have a reversed dip.

April 17th.—The second part of a memoir, commenced on the 27th of March, and entitled, “On the sedimentary deposits which

occupy the western parts of Shropshire and Herefordshire, and are prolonged from N.E. to S.W., through Radnorshire, Brecknockshire, and Caermarthenshire, with descriptions of the accompanying rocks of intrusive or igneous characters," by Roderick Impey Murchison, Esq. F.G.S. F.R.S. &c., was read.

In this part of the memoir the author separates into distinct formations by the evidences of fossils and the order of superposition, the upper portions of those vast sedimentary accumulations, which have hitherto been known only under the common terms of transition rocks, and grauwacke. Commencing at the base of the old red sandstone, which formation he had described in the previous part of the memoir, he proceeds to give an account of the underlying deposits as they succeed to each other in descending order in Shropshire and Herefordshire.

I. *Upper Ludlow Rock*.—Equivalent, Grauwacke Sandstone of Tortworth, &c.

This group, so named because the Castle of Ludlow stands upon it, is as eminently characterized by the presence of organic remains as the old red sandstone is by their deficiency. Amid a profusion of fossils, the upper beds are characterized throughout the whole range of the formation by two species of *Strophomena* or *Leptæna*, an *Orbicula*, a plicated *Terebratula*, &c. all of undescribed species. The middle beds contain many species of *Orthocerata*; *Serpulæ*? of great size, &c.; and the lower strata are charged with a profusion of small *Terebratulæ* having a gryphoid form. *Trilobites* of the genera *Homonolotus* and *Calymene* occur. The group has a maximum thickness of about 1000 feet, is for the most part a thin-bedded sandstone, often highly calcareous, and at other times argillaceous, and in Shropshire frequently occupies distinct lofty ridges interposed conformably between the old red sandstone and the inferior limestone.

II. *Wenlock Limestone*.—Equivalents, Dudley Limestone, Transition Limestone, &c.

On the banks of the Severn near Wenlock, and in the Wenlock Edge, this group is particularly abundant in corals and *Encrinites*, nearly all the species of which, as well as of certain *Mollusca*, are found in the well-known limestone of Dudley. The exact position, therefore, which the latter occupies in the geological series of England, is thus for the first time determined.

The upper beds rising from beneath the Ludlow rock are thin-bedded and lenticular, and the lower beds in Wenlock Edge contain many concretions of very great size and of highly crystalline structure. Throughout its course in the district included between the rivers Oney and Lug, this limestone is chiefly characterized by the abundance of one species of *Pentamerus*, and at Aymestrey it is rich in that and other fossils*. 100 feet are considered to be about the thickness of this calcareous zone.

* The author cannot allude to the fossils of this district without expressing his deep obligation to the Rev. T. Lewis of Aymestrey, whose unceasing researches have contributed very essentially towards the zoological illustration of this memoir.

III. *Lower Ludlow Rock*.—Equivalent, “Die Earth.”

This group is chiefly made up of incoherent, grayish, argillaceous schist, seldom micaceous. The higher strata are in some places charged with many *Orthocerata* of new and undescribed species, *Lituities*, *Asaphus caudatus*, &c. Other beds are locally distinguished by concretions of argillaceous limestone, formed around corals and other organic bodies; and towards the base of this deposit a thin calcareous zone is observable in Shropshire, containing the *Pentamerus lævis* and a new species of that bivalve, both differing from the species noted in the overlying group 2. The thickness is supposed to exceed 2000 feet.

The dislocations and faults on the Severn are described, by which this unproductive shale or “Die Earth” is brought in one point into unconformable contact with, and in another passes conformably beneath, the coal fields of Madeley and Brosely.

IV. *Shelly Sandstones*.—Equivalent, ————?

Red and green colours predominate in these sandstones, although purple and white are also frequently observed; by which characters, as well as by the nature of the stone and the specific differences in the organic remains, this formation is clearly distinguished from any of the overlying groups. Associated with the sandy strata are calcareous bands, almost made up of *Productæ*, *Leptænæ*, and *Spiriferi*, with crinoidal remains, all differing from those in the superior deposits. In Shropshire this formation rises at low angles from the valleys of lower Ludlow rock or Die Earth, and occupies separate ridges on the south-eastern flanks of the Wrekin and the Caer Caradoc. By a rough estimate 1500 to 1800 feet are assigned to the depth of the deposit.

V. “*Black Trilobite Flagstone*,” &c.—Equivalent, ————?

The prevailing Trilobite in this formation is the large *Asaphus Buchii*, which with the other associated species is never seen in any of the overlying groups. In the mountain called the Longmynd, this flagstone is made up of black schists, hard and dark-coloured grauwacke sandstone, &c., in which Trilobites have not yet been observed, although they are abundantly characteristic of the same zone in its prolongation through Radnor, Brecknock, and Caermarthen shires, where these fossils occur in black limestone and calcareous flagstone and grit. The thickness of this formation probably exceeds that of any one of the superior groups.

VI. *Red Conglomerate, Sandstone, and Slaty Schist*.

This is a vast deposit of several thousand feet in thickness, consisting of very coarse, quartzose conglomerates, which alternate with some schistose beds and much purple-coloured sandstone (Compound Sandstone of Townson), the strata of which in Haughmond, Pulverbatch, and Linley Hills, Shropshire, are highly inclined or vertical, in conformity with those of the preceding formation. No organic remains have been observed, by which, and by its very peculiar mineral structure, this formation is shown to be entirely distinct from the preceding groups.

The above six deposits are all exhibited in Shropshire, trending from N.E. to S.W., and occupying distinct ridges and valleys. In their further prolongation to the S.W. the upper Ludlow rock is uniformly persistent. Strata lithologically similar and containing the same fossils are found invariably to rise from beneath the old red sandstone in the counties of Hereford, Radnor, Brecknock, and Caermarthen; sometimes at very low angles of inclination, while at others, as in the promontories near Ludlow and Brecon, they are thrown up into saddles, and at the south-western limit of Brecknock and Caermarthen shires they are vertical or very highly inclined.

The second deposit, or Wenlock (Dudley) limestone, thins out a little to the S.W. of Aymestrey, and the groups 1 and 3 being brought together, generally occupy the same lofty escarpment in their course through S. Wales. Hence the author suggests the term Ludlow formation (the upper and lower Ludlow rock being subordinate members), as applicable to all the higher portion of this series which has a tripartite character in Salop and Hereford, due to the interpolation of the Wenlock and Aymestrey limestone.

The deposits 4, 5, and 6, are three separate formations, entirely differing from each other, and from the Ludlow formation, in their characters, mineral and fossil, and in the distinctness of their physical demarkations. They are not, however, to be traced continuously in their course from Shropshire on the N.E. to Caermarthenshire on the S.W.; though they reappear at intervals on that strike, preserving their relative places in the geological series.

In those districts where parallel ridges of all these formations are brought to day within a zone of small breadth, rocks of trappean or igneous origin are usual accompaniments, as in the neighbourhoods of the Wrekin and Caer Caradoc, in Shropshire; and again, after a long interval, in the environs of Old Radnor, Builth and Llandegley. In the intervening and featureless tracts of Clun, Knuckless and Radnor forests, where such intrusive rocks are absent, the Ludlow formation alone is spread out in undulating masses, and upon its surface are frequently found detached and elevated basins of old red sandstone.

The heights of the different groups above the sea-level vary from 500 to 2000 feet.

The author reserves for the third part of his memoir, which he proposes to communicate on a future occasion, the description of the numerous trappean and porphyritic rocks, which, in penetrating through these grauwacke deposits, have produced changes in their mineral aspect and structure. On that occasion the question of the parallelism of these sedimentary groups will be reviewed in reference to the direction of the outbursts of rocks of igneous origin. The quartz rock on the flanks of the Wrekin and Caer Caradoc, and also in the singular ridge of the Stiper Stones, will be described under the head of "Altered Rocks." The relations of the formations on the eastern side of Herefordshire will also be explained, with the view of determining whether deposits of the same age and charac-

ter rise from beneath the old red sandstone in the flanks of the Malvern Hills, &c., as have been shown to exist on the opposite or western side of the great field of old red sandstone.

May 1.—A paper was first read, entitled, “Notice of a Machine for regulating high temperatures, invented by the late Sir James Hall, Bart., F.G.S.,” and drawn up by Captain Basil Hall, R.N., F.G.S. &c.

A letter was afterwards read from Mr. Telfair to Sir Alexander Johnstone, V.P.R.A.S., accompanying a specimen of recent conglomerate rock, from the Island of Madagascar, containing fragments of a tusk, and part of a molar tooth of a hippopotamus; and communicated by Roderick Impey Murchison, Esq. F.G.S.

ROYAL ASTRONOMICAL SOCIETY.

May 10.—The following communications were read:—

On the Latitude and Longitude of the Cape Observatory. By Mr. Henderson.

In this paper Mr. Henderson assumes the geographical position of the Cape Observatory to be $33^{\circ} 56' 3''$ south latitude; $1^{\text{h}} 13^{\text{m}} 56^{\text{s}}$ east longitude from Greenwich; the former being determined from his own observations, and the latter from those of Mr. Fallows compared by Mr. Henderson with the corresponding observations made at European observatories. From the determination of the longitude of the Cape Observatory here stated, compared with observations made by Lieut. Meadows and by Capt. Owen, Mr. Henderson also finds that the longitudes of the two Capes which inclose False Bay may be stated as follows:—

Cape of Good Hope $18^{\circ} 29' 0''$

False Cape $18^{\circ} 50' 30''$ east of Greenwich.

“This longitude of the Cape Point,” Mr. Henderson observes, “differs only one mile from that given by La Caille ($16^{\circ} 10'$ east of Paris—*Memoirs of Academy of Sciences* for 1751, p. 425); and it is but justice to the memory of that distinguished astronomer to remark, that the lapse of eighty years, and the superior means which the present state of the science affords, have not been able to produce any improvement in the geographical position of this part of the world, as determined by him.”

Positions of Stars near the South Pole, from Observations made at the Observatory at the Cape of Good Hope; also by Mr. Henderson.

There being no conspicuous star near the South Pole, which can be observed in the day-time, even with the most powerful meridian telescopes that are used in observatories, it becomes necessary, in the southern hemisphere, for the purpose of determining the polar positions of astronomical instruments, to have recourse to stars of less magnitude than those which are observed for that purpose in the northern hemisphere, and even to increase their number, in order that one or more of them may pass the meridian at a convenient time of the night. Since Mr. Henderson's arrival at the Cape Observatory, in April 1832, seven circumpolar stars have been frequently observed with Dollond's 10-foot transit telescope, and Jones's 6-foot mural circle, for the purpose of determining the positions of those instruments with regard

to the Pole. Of the stars whose places have been thus determined, believing that their positions may be of considerable use to astronomers, Mr. Henderson now communicates a Catalogue, exhibiting their mean positions reduced to the beginning of 1832 (fictitious year of the Astronomical Society's Catalogue). This Catalogue, under the title of "Positions of Stars near the South Pole for the beginning of 1832," is given in the "Monthly Notices" of the Society for May.

Various Observations made at the Cape Observatory, by Mr. Henderson, Mr. Meadows, and Mr. Fayer : viz. observed right ascensions of the moon and stars 1832, April 10 to October 17, inclusive ; observations of the moon and stars, made with the mural circle, for the determination of the moon's parallax ; 1832, July 20, to January 2, 1833,—of Mars and stars, made with the mural circle, for the determination of the parallax of Mars ; eclipses of Jupiter's satellite, 1832, April 12 to October 19 ; occultations of planets and fixed stars by the moon, 1832, August 10 to October 31 ; and the transit of Mercury, May 4 and 5, 1832.

A paper on the Solar Parallax. By Mr. Shires.

On the Determination of the true Heights of distant Mountains from their apparent elevations above the sea. By G. Blackburne, B.A.

The object of this investigation is to furnish a convenient formula for the computation of the terrestrial refraction in terms of the distance of the observer from the object, and the apparent elevation ; and secondly, for the computation of the true height of the object itself.

June 14.—The following communications were read, some of which we shall further notice in our next Number.

I. Observations of Biela's Comet, from Nov. 18, 1832, to Jan. 13, 1833. By T. Henderson, Esq., Astronomer at the Cape of Good Hope.

II. Various Observations made at the Observatory of the Cape of Good Hope. Communicated by T. Henderson, Esq.

This series of observations, made at the Cape Observatory by Messrs. Henderson and Meadows, is a continuation of those already communicated, as noticed above.

III. Observations of *Mars*, and Stars observed with *Mars*, near the Opposition, between Oct. 14, and Dec. 22, 1832, at the Observatory, St. Helena. By Lieut. Johnson.

IV. Observations of the Magnitude, Colour, and Brightness of Stars in the Southern Hemisphere. By James Dunlop, Esq., Astronomer at Paramatta.

V. Observations of several Meridian Transits of the Moon. By the Rev. M. Ward.

VI. Observations of Occultations and Eclipses made in the year 1832, at Biggleswade, with the Wollaston Telescope. By T. Maclear, Esq.

VII. Description of a small Observatory erected at Biggleswade. By T. Maclear, Esq.

VIII. An Approximate Method of finding the Latitude from two Altitudes observed near the Meridian, with the interval of time. By Lieut. Raper.

IX. Notice of the Elliptic Orbit of ξ Boötis, with a further Approximation to the Orbit of γ Virginis. By Sir John Herschel.

X. On the Adoption of a Standard of Optical Power by Observers. By the Rev. W. R. Dawes.

XI. Description of a Double Reflecting Circle, and of a Quadruple Reflecting Sextant, made by Mr. Jones, of Charing Cross, under the direction of Captain W. F. Owen, R.N., and presented by the latter to the Society.

XII. Transits of the Moon with Moon-culminating Stars observed at Cambridge Observatory, in the months of March, April, May, and June, 1833.

Agreeably to the intention expressed in vol. ii. p. 476, we now return to some prior meetings of the Society, during the present year, which have not hitherto been noticed in the Philosophical Magazine, in order to complete our calendar of its proceedings.

February 8.—At the Annual General Meeting holden this day, being the Thirteenth, the Report of the Council was read, the Gold Medal was presented to Professor Airy, for his paper “On an irregularity of long period in the motions of the Earth and Venus,” published in the Philosophical Transactions for 1832, (of which an abstract was given in Phil. Mag. and Annals, N.S., vol. xi. p. 117: see also Lond. and Edinb. Phil. Mag., vol. i., p. 61.) and the following Fellows were elected the Council and Officers for the ensuing year: viz.

President: Francis Baily, Esq., V.P.R.S.—*Vice Presidents*: George Biddel Airy, Esq., M.A., Plumian Prof. Ast. University of Cambridge; Captain W. F. Owen, R.N. F.R.S.; Davies Gilbert, Esq., F.R.S.; John William Lubbock, Esq., M.A. V.P. and Treas. R.S.—*Treasurer*: John Lubbock, Esq., LL.D.—*Secretaries*: George Bishop, Esq.; Augustus De Morgan, Esq.;—*Foreign Secretary*: Captain W. H. Smyth, R.N. F.R.S.—*Council*: Bryan Donkin, Esq.; Lieut. Thomas Drummond, R.E.; Thomas Jones, Esq.; Rear Admiral Sir Edward Owen, K.C.B.; Lieut. Henry Raper, R.N.; Edward Riddle, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; Robert Snow, Esq., B.A.; Lieutenant William S. Stratford, R.N. F.R.S.; Thomas Turner, Esq. M.A.

April 12.—(continued from vol. ii. p. 475.) Of the researches detailed and discussed in Professor Airy's paper “On the Mass of Jupiter” read at this meeting, we have already given a summary account, at p. 314 of our last volume. Some particulars of the numerical errors discovered by the author in the *Mécanique Céleste*, and his final statement and estimate of the value of his own results, we now subjoin:

“I had at first no intention of correcting, or even examining the numbers of the *Mécanique Céleste*; but an examination simply for the verification of the typographical accuracy, led me to doubt the correctness of several of the numbers in page 94 of the fourth volume, and of those which depend on them. On further examination, I found that the variable terms, both of the radius vector and longitude, as far as I examined, were nearly correct; but that in the constant parts of the radius vector some terms were wrong in sign, one erro-

neous in magnitude, and one omitted. I corrected these, and then proceeded in the construction of my tables."

"In the first instance, however, to which I applied the calculations, I found, to my great astonishment, that, at the time when I had observed the satellite at extreme elongation, the tables represented it as between the earth and *Jupiter*. I satisfied myself that this was owing to no error in my own calculations, and I then compared Laplace's numbers with those of Halley and Delambre. From the tables of the former I could only infer that there was some very strange error in Laplace's epoch for 1750; from the mean conjunctions of the latter I could calculate its quantity. It will scarcely be believed, that the epoch of mean longitude in page 139 of the fourth volume of the *Mécanique Céleste* is erroneous to the amount of nearly one third of the circumference, and that this is not a typographical error, as several numbers are combined with it by addition and subtraction in the pages following it, and all correctly. I confess that my faith in the numerical results of the *Mécanique Céleste* has been very much weakened by these discoveries."

"At the same time, I am far from imputing any negligence to the author of that great work; for I know too well the difficulty of insuring correction in long calculations from complex formulæ: and in some of the fundamental numbers of the present computations I was saved from error only by Mr. Lubbock's re-calculations."

A list of the errata in the *Mécanique Céleste* is appended to the memoir.

Returning to Professor Airy's own researches, we find that

"After applying two small corrections, one for the clock, and the other for obtaining the true mass of the Jovial system, Professor Airy concludes, that we have for the mass of the Jovial system 6.9793486, and for the mass $\frac{1}{1048.70}$

"In conclusion I may remark, that I do not think this value liable to any serious error, except from a possible error in the inclination of the orbit of the fourth satellite. The effect of the greatest error of the instrument in azimuth (0.5) that any observations have shown, acting at the greatest disadvantage, would not alter the denomination of the fraction by 0.2. But the observations used here have been made when *Jupiter* was in the most unfavourable of all positions, both from the effect of such errors, and from the smallness of the difference of right ascension, which is the immediate subject of the measures. The oppositions of 1834-5 and 1836 will be very much more favourable."

"I am far from regarding the question as definitively settled by these measures; and I offer these results to the Society as only the first fruits of my investigations. Should the regular business of the Observatory, and my other employments, leave me sufficient leisure, I hope to add considerably to their number. The necessary tables and forms of calculations are prepared; some theoretical and numerical difficulties have been surmounted; and the labour of calculating future observations will therefore be very much less than that of calculating the first."

PHILOSOPHICAL SOCIETY OF CAMBRIDGE.

May 20.—The following communications were read:—

On the Attraction of Spheroids, by G. Green, Esq. In this paper the author presents certain analytical formulæ, in reference to triple integrals of a more general form than those offered in the attractions of spheroids of arbitrary form and density, and applies them to the problem of the attractions of ellipsoids, so as to comprise the actions on points, internal and external, in a common process, by the addition of a positive quantity, under the radical sign in the expression, for the reciprocal distance between the point acted on and any point of the ellipsoid, which quantity is afterwards made to vanish.

On the Determination of the Vibratory Motion of Elastic Fluids in Tubes of definite Length, by W. Hopkins, Esq. of St. Peter's College. The author described a series of experiments made by him with the view of subjecting to an experimental test the different solutions which have been given of this problem. The intensity of the vibrations in any part of the tube are indicated to the eye by the motion which those vibrations excite in a delicate membrane, sprinkled with light sand, and suspended in the tube. The positions of the nodal points, thus determined with great accuracy, are not such as accord with any solution of the problem hitherto given; but it was shown how all the observed phenomena are accounted for by the assumption of certain physical conditions more general than those assumed by previous writers.

An experiment was also exhibited by Mr. Hopkins, showing the effect of interference of two aerial undulations proceeding in the same direction. The ends of two equal tubes, branching off from one common tube, were placed close to two ventral segments of a vibrating plate, by which the vibrations are excited in the branch tubes. The vibrations were in the one with which they communicate. If the vibrations proceeding from the two ventral segments be in the same phase, the resulting vibration is one of great intensity; but if they are in opposite phases no sensible vibration results from them. The intensity of the vibration is indicated, as above mentioned, by a membrane which may be stretched over the mouth of the tube.

XLII. *Intelligence and Miscellaneous Articles.*

COMPOUNDS OF CHROMIC ACID WITH METALLIC CHLORIDES.

M. E. PELIGOT has formed several compounds of chromic acid with metallic chlorides. The bichromate of chloride of potassium is easily and economically produced by boiling a mixed solution of bichromate of potash and muriatic acid for some time; on cooling, a quantity of the salt in question crystallizes, in proportion to the quantities of ingredients employed.

According to M. Peligot the theory of the formation of this salt is as follows: the muriatic acid at first acts upon the potash of the bichromate, and forms chloride of potassium, and water, and at the

same time free chromic acid is developed. If, when all the potash has been converted into chloride of potassium and water, the solution is allowed to cool, it soon deposits large crystals of bichromate of chloride of potassium; so that all the bichromate of potash is converted into water and bichromate of chloride of potassium before the muriatic acid, supposed to be in excess, produces protochloride of chrome, which only happens by the decomposition of the salt which is formed.

The atomic reaction is thus given :—

Atoms employed.		Atoms produced.	
2 atoms chromic acid	1304	2 atoms chromic acid.....	1304
1 atom potash.....	589	1 atom chloride of potassium	931
2 atoms muriatic acid	454	1 atom water	112
2347		2347	

The principal properties of this salt are, that it crystallizes very easily; the crystals are right prisms with rhombic bases; their colour is similar to that of bichromate of potash; they do not deliquesce. The action of water on this salt is remarkable: when a crystal is moistened with it, it becomes white and opaque; and if the solution be suffered to evaporate spontaneously, no crystals of the salt in question are obtained; but merely those of bichromate of potash. This latter salt is, indeed, produced whether the liquor be concentrated by heat, or exposed to spontaneous evaporation. It appears, then, that water is decomposed by the bichromate of chloride of potassium: its hydrogen combining with the oxygen to form muriatic acid which becomes free, while the potash remains the potassium.

This reaction produces precisely the products employed in the formation of the bichromate of the chloride, and the result of the operation will be shown by transposing the atoms above given.

Atoms employed.		Atoms produced.	
2 atoms chromic acid	1304	2 atoms chromic acid . .	1304
1 atom chloride of potassium	931	1 atom potash	589
1 atom water	112	2 atoms muriatic acid . .	454
2347		2347	

By the addition of a sufficient quantity of muriatic acid, to correct the oxidizing action of the water, the bichromate of the chloride is reproduced. On account of this action of the water, it is impossible to purify this salt by crystallization: it is requisite to press it between folds of blotting paper: it almost always contains a slight excess of chlorine.

The most economical process for obtaining this salt has been already stated; it may also be obtained, and in a more direct manner, by mixing 2 atoms of chromic acid with 1 atom of chloride of potassium, provided the solution be rendered acid by muriatic acid. It may be procured also by treating bichloride of chrome with water saturated with chloride of potassium; and in this case all the

conditions favourable to its formation are fulfilled, for the water converts the bichloride of chrome into chromic and muriatic acid. If neutral chromate of potash be used instead of the bichromate, the crystals obtained are mixed with crystals of chloride of potassium.

The following process of analysis was preferred and employed on account of its simplicity :—A portion of the salt, dried between the folds of blotting paper, was dissolved in distilled water acidified by nitric acid, and nitrate of silver was then gradually added, and the chloride obtained was washed, dried and weighed. The liquor, after the separation of the chloride of silver, was boiled with sulphurous acid; this converts the chromic acid into protoxide of chrome, and is itself converted into sulphuric acid: ammonia then precipitates protoxide of chrome by long boiling. The potash was then converted into sulphate by means of sulphuric acid, and its quantity thus determined.

The bichromate of muriate of ammonia resembles that of chloride of potassium in appearance and crystalline form, but it is much more soluble in water. By analysis it yielded—

Chromic acid		65.5
Muriatic acid		23.5
Ammonia		10.8

99.8

got found that the bichromates of the chlorides of sodium, and magnesium were deliquescent: he was unable to produce a precipitation of the chlorides of strontium and barium, on the addition of their salts from water by the muriatic acid. *Chim. et de Phys.* lii. 267.

ON FOSSIL-BONE CAVERNS.

Journal, jun., of Narbonne, concludes from a great variety of facts and observations, that,

1st, The bones buried in caverns have been introduced in several different manners.

2ndly, The species buried vary in different localities; and this difference depends either upon the period of deposition or the geographical position of the cavern.

3rdly, Man was contemporary with the lost species of animals which are found buried in the mud of the caverns; and these animals being regarded by all naturalists as fossil, man therefore exists in the fossil state.

4thly, The mud and pebbles found in the bone caves were not introduced by any sudden and temporary cause, as by a deluge, but on the contrary, almost always slowly, and in several different modes.

5thly, The attentive examination of the pebbles contained in the mud proves evidently that they have been introduced from neighbouring places, and consequently, that the cause which conveyed them was entirely local.

6thly, The period during which the bone caves were formed was extremely long.

7thly, In certain caverns, the mud, bones, and pebbles have been simultaneously introduced; in others, on the contrary, the mud has been deposited after the bones.

The bones of the following animals have been found in the caverns of the South of France:—

Elephant.	Goat.	Pole-cat.
Rhinoceros.	Sheep.	Hare.
Wild Boar.	Bear, at least two	Rabbit.
Horse.	species.	Lagomys.
Ox, two species.	Badger.	Campagnol.
Stag, five species.	Tiger.	Birds, several species.
Antelope, very large.	Lion.	Land Tortoise.
Chamois.	Leopard.	Lizard (<i>Lacerta ocellata</i>).
Hyæna, fossil.	Lynx.	Snake, size of the
Hyæna, striped.	Wolf.	<i>Coluber Natrix</i> .
Hyæna, brown.	Fox.	
Dog, two species.	Weasel.	

The only difference which exists between the caves of England and Germany, compared with those of the South of France, is in England the hippopotamus has been found, and at Saxony Westphalia, the glutton.—*Ann. de Chim. et de Phys.* li.

ON THE VISIBILITY OF STARS

Sir John Herschel, in his popular Treatise notices the fact of stars being distinctly visible in day-light by telescopes, their brightness being proportional to the power of the instrument; but, which is unusual with an author, he omits to explain the reason.

If the following be not the true reason, perhaps correspondents of the Philosophical Magazine will be kind enough to correct me.

The rays of day-light which enter the object-glass of a telescope must, from our proximity to the points of their reflection, be much more divergent than the rays of a distant star; consequently none of the former rays can be converged to so short a focus as the latter. Hence, if a telescope be so adjusted as to bring the “parallel” rays from a distant star to a focus, none of the divergent rays from the nearer atmosphere can be condensed at the same or so short a focus, and the atmospheric light within the tube cannot much interfere with the concentrated light of the star. As we cannot, while looking through a telescope at a distant ship, see the flame of a candle, or cross wires, held immediately before the object-glass, by reason of the nearer flame or wires’ requiring a longer focus than the distant ship, so for the same reason we cannot, while looking through a telescope at a distant star, see the nearer light of the atmosphere which partially intercepts it. In either case that which intervenes produces no other effect than a partial obscuration of the distant object.

Sir J. Herschel notices also the fact of bright stars in the zenith being visible in the day-time, even by the naked eye, from the bottoms of deep narrow pits, or through the shafts of chimneys. May not this be owing to the divergent rays of the atmosphere spreading and losing themselves against the non-reflecting sides of the pit or shaft, so that few or none of them ever reach the bottom; while, on the contrary, all the rays from the star which enter the vertical pit proceed to its bottom without the least sensible divergence?

Redruth, July 13, 1833.

RD. EDMONDS Jun.

LEYBOURN'S MATHEMATICAL REPOSITORY.

The twenty-fourth Number of this valuable Work is just published, and contains the following articles:—

1. *Nugæ*, or Constructions to represent approximately the length of the circumference of a Mile.

2. Solutions of the twenty Mathematical Questions proposed in No. 22; by various Contributors.

3. New Solution to Question 531 (relating to the *Αρσηνλον* of *Archimedes*), and some collateral properties of the figure; by T. S. Esq. F.R.S. Lond. & Ed.

of the late Sir John Leslie, K.H.

g to the Mathematical Parts of the Philosophical Transactions of the Royal Society of Edinburgh, of the Cambridge Philosophical Society, and *Polytechnique*.

Problems; by James Cunliffe, Esq.

at to the Lunar Theory; by the Rev. Brice Bron-

the Computation of Surfaces and Solids bounded by lines passing through points, by means of the coordinates of those points; by Mr. Woolhouse.

9. New Researches in Spherical Trigonometry; by T. S. Davies, Esq.

10. On the Deviation of two Curves, in answer to a note of Mr. Woolhouse, inserted in the last Number; by T. S. Davies, Esq.

11. On a partial Differential Equation; by W. Sutcliffe, Esq. A.M., Bath.

12. On the Figure of a homogeneous Mass of Fluid revolving about an axis with a given uniform Velocity; by the Rev. Brice Bronwin.

13. Translation of Dr. Matthew Stewart's Extension of Prop. 4. Lib. iv. of the Mathematical Collections of Pappus (concluded).

14. Certain Properties of Plane Triangles not generally known; by C. F. A. Jacobi.

15. Twenty new Mathematical Questions for solution in No. 26; by various Contributors.

Pennance, and Mr. V. FALL at Boston.

Days of Month, 1893.	Barometer.				Thermometer.				Wind.			Rain.			Remarks.
	London.		Penzance.		London.		Penzance.		Wind.	Dir.	Force.	Inch.	Inch.		
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.							
July 1	29.873	29.831	29.916	29.819	66	40	58	48	W.	NW.	W.	0.20	0.280	...	London. — July 1. Fine: heavy rain, and a hail shower about 7 P.M.: clear at night.
2	29.998	29.919	29.969	29.966	76	45	58	48	SW.	NW.	W.	...	...	...	2. Fine. 3. Fine: thunder. 4, 5. Fine.
3	30.121	30.014	30.116	29.966	69	44	58	47	W.	W.	NW.	...	...	...	6. Very dry. 7. Cloudy: sultry with showers.
4	30.178	30.168	30.113	30.033	75	51	65	54	W.	W.	calm	...	...	...	8. Heavy rain at noon: cloudy and rather windy at night. 9—11. Fine. 12. Dry haze:
5	30.167	29.997	30.013	29.904	76	49	66	55	SW.	SE.	calm	...	...	...	fine. 13. Overcast: fine. 14—19. Very fine.
6	29.857	29.656	29.657	29.560	77	50	63	55	SE.	NW.	SE.	.01	.200	...	20. Fine: cloudy: showery. 21. Cloudy: showery: fine. 22. Rain: clear.
7	29.767	29.612	29.660	29.560	68	55	64	50	S.	NW.	calm	.20	...	...	23. Cloudy: rain. 24—26. Very fine.
8	30.022	29.808	29.954	29.860	71	52	66	51	W.	NW.	E.	.24	...	...	27. Very hot: thermometer 86° in shade.
9	30.108	30.080	30.107	30.104	73	45	58	55	N.	NW.	NE.	...	...	...	28, 29. Very hot. 30. Cloudy: fine.
10	30.031	29.959	30.104	30.004	76	51	69	56	NW.	NW.	calm	...	...	...	31. Overcast: clear and fine.
11	29.992	29.894	30.004	30.004	69	53	64	55	N.	NW.	E.	...	...	...	Penzance. — July 1. Showers. 2. Fair.
12	29.899	29.852	29.857	29.857	61	50	60	53	NE.	NW.	calm	...	.125	...	3. Misty: fair. 4, 5. Fair. 6. Fair: rain.
13	29.921	29.854	29.910	29.904	64	47	65	55	SW.	SW.	calm	...	...	...	7. Fair. 8, 9. Clear. 10, 11. Fair. 12. Rain:
14	30.002	29.958	29.907	29.854	73	54	63	53	SW.	SE.	calm	...	...	...	fair. 13. Fair. 14. Fair: rain. 15. Fair.
15	30.131	30.006	30.054	29.910	75	45	65	53	NE.	NE.	calm	...	...	...	16, 17. Clear. 18. Clear: fair. 19, 20. Clear:
16	30.211	30.194	30.154	30.110	79	50	68	53	E.	NE.	calm	...	...	...	showers. 21. Fair. 22. Showers: fair.
17	30.226	30.194	30.154	30.148	84	55	70	55	NW.	NE.	calm	.02	...	...	23. Rain: fair. 24—31. Clear.
18	30.162	30.011	30.148	30.048	81	57	70	54	W.	SE.	calm	...	.055	...	Boston. — July 1—4. Fine. 5. Cloudy.
19	29.906	29.772	29.948	29.848	76	57	66	57	SW.	NW.	W.	.18	.060	...	6, 7. Fine. 8—10. Cloudy. 11. Fine.
20	29.671	29.618	29.804	29.804	70	48	63	55	NW.	NW.	NW.	...	...	...	12, 13. Cloudy. 14, 15. Fine. 16, 17. Cloudy.
21	29.877	29.778	29.904	29.860	70	46	63	55	NW.	NW.	NW.	.05	...	...	18. Fine. 19. Cloudy. 20. Fine. 21. Fine:
22	29.835	29.697	29.857	29.798	73	54	65	55	SW.	SW.	W.	.26	.040	...	rain P.M. 22. Cloudy. 23. Fine. 24. Fine:
23	29.912	29.638	29.945	29.704	70	48	64	55	W.	W.	W.	.40	.130	...	rain P.M. 25. Fine. 26, 27. Fine: rain P.M.
24	30.196	30.024	30.098	30.024	75	48	65	55	W.	W.	W.	...	...	...	28, 29. Fine. 30, 31. Cloudy.
25	30.327	30.281	30.210	30.198	77	45	67	55	W.	W.	W.	...	...	...	
26	30.311	30.250	30.204	30.198	80	52	67	55	W.	W.	W.	...	...	...	
27	30.265	30.255	30.204	30.192	86	55	67	55	W.	W.	W.	...	...	...	
28	30.287	30.257	30.198	30.166	82	55	67	55	W.	W.	W.	...	...	...	
29	30.328	30.308	30.172	30.166	79	55	67	55	W.	W.	W.	...	...	...	
30	30.458	30.423	30.246	30.212	79	55	67	55	W.	W.	W.	...	...	...	
31	30.429	30.351	30.292	30.286	79	55	67	55	W.	W.	W.	...	...	...	
	30.458	29.612	30.292	29.560	86	55	67	55	W.	W.	W.	1.46	1.810	0.60	

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[THIRD SERIES.]

NOVEMBER 1833.

LIV. *On the Vibration of heated Metals.* By ARTHUR TREVELYAN, Esq.; including a Letter on the same Subject by Dr. W. KNIGHT.*

AS it is always interesting to know the accidental circumstances which lead to the discovery of any new fact in natural science, I shall begin by relating the manner in which I was led to observe the vibrations of heated metals. On the 9th of February 1829, I was about to spread with a plaster-iron some common pitch, when the iron being at too high a temperature, I placed it in a slanting direction against a block of lead, the handle resting on the table: shortly after doing so I heard a shrill high tone, which much resembled a note produced on the chanter of the smaller Northumberland pipes, an instrument played on by my father's gamekeeper. Not knowing the cause of the sound, I thought he might be practising out of doors. The room being on the ground floor, I went outside, when the sound was no longer heard; and seeing no person, returned again to the room, when I was surprised at still hearing the sound as shrill as before. After looking about for a few minutes, I at last approached closer to the iron, perceived the vibration, and thence found the immediate cause of the sound: I was much surprised and pleased; but at that time, not having studied any branch of natural philosophy connected with the subject of heat, I was not aware that it was a fact which had not been before observed (although it struck me so at the time). I came to Edinburgh in November 1830, and attended Dr. D. B. Reid's class of

* Communicated by the Author. Part of this paper was read before the Section of Physics of the British Association, meeting at Cambridge, June 27, 1833.

Practical Chemistry. Having mentioned the circumstance and shown him the experiments, he informed me that the fact was new, and recommended me to make further experiments, and prepare a paper on the subject for the Royal Society of Edinburgh. I took his advice, and during the winter session had the honour of having two papers read before that Society, which were illustrated by experiments.

Since that time I have made numerous additional experiments, and have succeeded in obtaining vibration with most metals, many of which I failed with previously, from the apparatus not being sufficiently delicate.

On the opposite page is a Table of the different metals in which I have observed vibration.

The experiments were repeated several times before I was satisfied with the results.

I have not yet observed any effect with bismuth, although I have with pale solder, which is an alloy of that metal.

The gold was in too small a piece to try cold, with the hot bar placed on it. The results with it and with platinum would probably have been more numerous, if I could have obtained better-shaped pieces of those metals.

Bars of tin, lead, bismuth, antimony, block tin, solder, tin-solder and pale solder, produced no effect, when heated and placed on cool blocks, or rings, of all the different metals mentioned in the Table.

Cold bars of lead, bell-metal, tin-solder and pale solder, when placed on heated iron or brass, produced vibration and tone. The cold lead block, placed on the heated polished bar of a fire-grate, sounds loudly, and vibrates rapidly.

The vibration continues in the exhausted receiver of an air-pump.

The bars begin to vibrate on lead at a temperature below 212° Fahr., but on harder metals they require a higher temperature.

A cool brass bar, 5 inches long, 2 inches wide and $\frac{3}{8}$ ths of an inch thick, placed on a cool lead cylinder $\frac{1}{2}$ an inch thick, 2 inches in height and $4\frac{1}{4}$ inches in diameter, with a spirit-lamp placed under the bar, produced vibration and tone in 6 minutes 15 seconds, and continued sounding for 5 hours and 55 minutes: when I removed the lamp, it ceased vibrating in 6 minutes, but might probably be continued, by the continued application of the lamp, for any length of time. The block had arrived at so high a degree of temperature, that it was too hot to hold with the naked hand: the vibration was only kept up by the brass bar being so superior to lead in its power of conducting caloric. From the small size of the bar, it soon fell to the same temperature as the lead, and then of course the vibration ceased.

Bars,	Simple Metals.										Alloys.							
	1. Plati- num.	2. Gold.	3. Silver.	4. Copper	5. Iron.	6. Cast Iron.	7. Zinc.	8. Tin.	9. Anti- mony.	10. Lead.	1. Fine Brass.	2. Com. Brass.	3. Gun Metal.	4. Bell Metal.	5. Cock Metal.	6. Block Tin Solder.	7. Tin Solder.	8. Pale Solder or Fusible Metal.
Platinum			.				+	+		+				+	+	+		
Gold										+	+	+		+	+			
Silver.....	+				+	+	+	+	+	+	+	+	+	+	+	+	+	+
Copper.....	+		+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Iron.....							+	+	+	+				+				+
Cast Iron								+		+								
Zinc.....			.					+	+	+				+			+	+
Fine Brass.....				+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Common Brass							+	+		+		+	+	+	+	+	+	
Gun Metal.....								+		+			+	+		+	+	
Bell Metal.....	.									+							+	
Cock Metal.....		.								+				+				

The crosses denote that vibration took place when the heated bar was placed on the cold metal named at the top of the column. The names of metals at the side of the Table are those of the bars which were heated and placed on the cold metal.

When the brass bar at a high temperature is placed on the cool lead, the vibration seldom fails taking place instantly and spontaneously.

A bar 5 inches long, 2 inches wide, and $\frac{3}{8}$ ths of an inch thick, vibrated when supporting a weight of 12 pounds.

However near the bar and block may be brought to each other, no apparent phenomenon takes place until they are in actual contact.

By balancing the bar in a horizontal position on a narrow lead block (fig. 1. p. 332), rounded on the part on which the bar rests, the vibration is well exhibited, the bar moving vertically and laterally at the same time. A rod 10 or more inches long, flattened in the centre to prevent its slipping, with a ball on each end (fig. 5), when placed across a heated vibrating bar (fig. 6), increases the arc of motion, rendering the movements much more conspicuous.

A thick ring of copper, 5 inches in diameter, when heated, and hung on a lead bar, vibrates backwards and forwards; and when laid on a narrow lead block, upwards and downwards.

The heated bar vibrated on a piece of thin sheet lead, either placed loosely or soldered on brass, and on a lead block burnished with gold-leaf.

A heated copper bar vibrated on the bottom of a glass tumbler. This is an experiment which the late Professor Leslie tried with me, but it is very uncertain and difficult to obtain.

The bars vibrate best when placed on blocks of lead with the surface somewhat rough: both metals also should be kept clean, and free from oxidation, which impairs the vibration.

A bar of heated copper was placed on an iron block rounded on the surface, and being nicely balanced on the centre of the rounded part, showed the vertical motion.

The shape or size of the bars and blocks is of little consequence, except for the more delicate experiments with the hard metals: on lead the hard metal almost of any form will vibrate when heated.

Mr. K. T. Kemp, the skilful electrician and chemist, informed me that after casting some bismuth, whilst it was cooling, after being taken out of the mould, he heard a sound proceed from it; when cool he again heated it, but could not reproduce the sound.

The bars I use to show the experiments are of different sizes: a bar about 5 inches long, $1\frac{1}{4}$ inch wide, and $\frac{3}{8}$ ths of an inch thick, will produce considerable tone; a rod of small wire 6 inches long, must be attached at one end, to serve as a handle. A ridge is formed along the centre of one side of the broad part, by its being bevelled off towards each edge; the other side is hollowed out down the centre so as to throw the

weight on each side, which assists the vibration: the longitudinal ridge is the part that rests on the block, (figs. 3 & 4.).

A hollow lead cylinder about $\frac{1}{2}$ an inch thick, with the diameter in proportion to the length of the bar, is the best form for showing the vibrations and producing the tone.

An inequality or unequal notch on the lead cylinder increases the sound. When the bar at a high temperature is first placed on the cold lead, the tone is very harsh and undefined; but when the lead has acquired a certain proportion of heat, it becomes clear, full and mellow.

Pressure on the bar alters the note: the greater the pressure the higher is the note. Pressure also applied to the sounding-board or table on which the vibrating bar may happen to be placed, or even walking across the room, by altering the position of the bar, changes the tone.

A common poker heated and placed on a lead block vibrates, producing deep tones. If the vibrating bar be placed on a piano-forte, and certain notes be struck on the instrument, the vibration of the bar, and consequently the tone produced by it, is altered, and sometimes suddenly stopped.

Vibration is prevented by rubbing the surface of the lead with mercury, oil, plaster of Paris, or by oil gilding; also if a piece of thin tissue paper, or of wire-gauze, be interposed between the bar and the block.

The vibrations have not been produced with any substances besides metals, except in the instance of the glass tumbler already mentioned.

To try whether there was any attraction between the hard and soft metals when heated, I had two pieces of brass, about the size of an ounce weight, with a wire attached to them, and then hung on a delicate scale beam: having heated the brass weights, blocks of lead were placed under them and tried at various distances, but without producing the slightest effect.

To try whether electricity caused the phænomenon or made any alteration whilst the bar was vibrating, a hole was drilled in a bar and filled with mercury, a wire was soldered to the lead cylinder on which it was placed, and a galvanic battery of 150 plates, 4 inches square, being charged, the wire from the cylinder was placed in one end of the battery, whilst from the other end a wire was brought to the mercury contained in the cavity of the bar, which was heated, and in motion, previous to completing the circle. We also tried other methods, but with none of them did we succeed in producing any change either in the tone or in the vibration.

A Leyden phial was charged with electricity, and applied to the same bar and block, without producing any effect.

Spirit of wine, or æther, evaporated off the heated bar when in motion, produced no effect.

Air blown with a pair of bellows on the heated bar when vibrating does not affect the tremors.

If the heated bar be ground smooth, on the resting part, and the block of lead be also very smooth, no vibration takes place.

When the bar and block become of the same temperature, the vibration and sound cease.

A cylinder, 5 inches long, with a thick side bevelled off, leaving a flat ridge in the centre, similar to the former bars, when heated and placed on lead, produced no change of tone.

From the above-related experiments it appears:

1st, That in order to produce the vibrations, metals must be employed, either of the same kind or different, for both the bars and blocks; with the single exception of the glass tumbler.

2nd, That the difference of temperature between the two metals must be considerable, although some require a much higher temperature than others: the vibrations on zinc and bell-metal succeed with a lower temperature than others of the hard metals.

3rd, That the surface of the block must have some degree of unevenness, for when rendered quite smooth, the vibration does not take place; but the bar cannot be too smooth.

4th, That the interposition of any matter prevents vibration, with the exception of a burnish of gold-leaf, the thickness of which cannot amount to the 200,000th part of an inch.

5th, That the air has no share in the production of the vibratory movements, however much its presence is essential to the production of sound.

6th, That it has no connexion with galvanism or electricity, the vibration and tone not being affected by passing a current through the bar when in action. Professor Forbes found no appearance of thermo-electricity after trying a number of experiments.

7th, That all the metals, both simple and alloys, produce vibration and sound, when one is heated and the other cool, on being brought into contact, (with the exception of bismuth,) but with some we find much greater difficulty in obtaining vibration than in others, as it depends on the temperature, but more particularly on the mode of placing the one on the other, which with some requires great nicety.

8th, Although all the metals are not found to vibrate on each other, or on metals of the same kind,—and I have not been able produce vibration with other substances,—yet I do

not despair but that hereafter, when this subject is better understood, we shall be able to produce vibration with all the metals, and with all other matter either solid or liquid, when heat is applied to a cool substance, or *vice versâ*.

The following theory, which is partly derived from the late Professor Leslie's mode of accounting for the vibration, and appears the most probable, (and experiments hitherto made strengthen this view,) ascribes the vibratory movements to the usual mechanical changes which caloric occasions in passing from one substance into another,—I mean the expansion and contraction which accompany alternations of temperature.

It appears that some degree of roughness of one metal is essential to the success of the operation. This slight asperity arises from numberless points or ridges projecting from the mass of metal.

When the heated bar is laid upon the cool lead, the caloric passes into these prominences; and as their power of conduction is not great, it does not rapidly diffuse itself through the rest of the mass: of course they instantly expand and elongate, and by that sudden elongation they give an impulse to the incumbent bar. Soon, however, the caloric moves into the adjoining mass, and the prominences contract, and at the same time come into a state ready to admit a renewed accession of caloric from the bar: they receive that caloric, again expand, and give a second impulse to the bar. This goes on incessantly, and though the first impulse be infinitely small, and altogether inadequate to produce any sensible movement of the bar, yet by incessant repetition an accumulation of effect takes place, and the movements gradually reach a sufficient magnitude to become easily discernible.

As soon as the bar and block arrive within a certain limit of difference of temperature, the impulses become feebler and feebler, and at length the bar comes to rest. It has been mentioned that the smoother the bar is so much greater is the effect: I conceive that this smoothness operates by increasing the celerity with which the surface possessed of it communicates the caloric to the projecting points of the block, and thereby the elongation which gives the impulse to the bar is increased both in quickness and extent.

It is obvious that had the bar any considerable degree of asperity, the points of contact between the two metals would be fewer, and the passage of caloric between them more tardy.

When the two surfaces are highly polished, the experiment does not succeed: no tremors occur. This result proceeds probably from the circumstance, that the caloric enters into every part of the surface of the block equally, and is more quickly diffused through the mass, and hence there are none

of those partial and sudden expansions which give the tremor-causing impulse.

The rocking on the narrow lead block may be induced by two causes, either by some slight inequality in the weight of the portions of the bar on the two sides of the ridge, or some difference in the condition of the surface of that part of the block which the ridge of the bar touches; and may be thus explained:

1st, If the first-mentioned inequality exist, as soon as the bar receives an upward heave, the greater weight of the one side will cause it to incline to that side; and as soon as that heave ceases, and the contraction succeeds, the bar approaches its original position, but will not remain in it, for the inclination given to the preponderating side will, on its return, of course, cause the bar to incline to the opposite side. The impulses which the bar receives, in this position, from the renewed expansion and elongation, will not only renew the upward heave, but also incline it again to the preponderating side, and thus increase the lateral movement, which, like the vertical, though altogether insensible at first, by incessant frequent repetition accumulates and increases till the rocking becomes conspicuous, and is rendered much more so by the transverse rod.

2ndly, If there exist any difference in regard to asperity in the condition of the surface of that part of the block upon which the ridge of the bar rests, it must necessarily follow, that the impulse given to the bar on that side which is most rough will be greater than on the other, and consequently the upward heave will be so modified as to create an inclination to one side. The bar thus thrown off its balance to the right on ascending, will incline as far to the left on descending, and there receiving the expansive impulse, it will be driven back, and thus the principle of rocking will be created. The sound depends upon the rapidity of the vibrations; for when slow, no tone is heard.

I think from the above-related experiments, and the conclusions and theory drawn from them, that the hitherto unknown causes of many sounds are now accounted for.

The sounds described by Humboldt as heard at sunrise by those who sleep on certain granite rocks on the banks of the Orinoco, also the sounds at sunrise produced by the statue of Memnon, and the twang, like the breaking of a string, heard by the French naturalists, as if proceeding from a mountain at Carnac, are probably caused by the pyrometric expansions and contractions of the heterogeneous material of which the statue and the mountain consist. Similar sounds, and from the same cause, are emitted, when heat is applied to any connected mass of machinery, and the snapping heard in a fire-grate

affords a familiar example. We have often heard of a poker producing tone when heated and the point rested on a knot in a fir board in the floor of a room; also the singing in a tea-kettle is another example. In distilleries, shortly after the fire is put to the cool coppers, a very loud note is given out, and continues until the liquor boils*.

John Robison, Esq., Secretary to the Royal Society of Edinburgh, informed me that he once let a heated bar fall from his hand: it alighted on a painted shelf of wood, when he was surprised at hearing sounds; but they soon ceased.

The following theory of the cause of earthquakes and volcanic eruptions strikes me as being not at all unlikely. Earthquakes, and the sounds accompanying them, may be caused by vibration, occasioned by heat generated far below the surface of the earth, in some enormous metallic mass, which being in contact with some cool substance, not a very good conductor of heat, the latter is violently agitated, thus producing the vibration felt in earthquakes. By its intensity chasms are opened on the face of the earth; and below, caverns filled with condensed combustible matter and liquid lava are torn open, and the contents, by their enormous expansion, and having found an egress more easily upwards, rise to a great height above the surface of the earth.

The following interesting remarks on the vibration of heated metals are copied from a letter received by my brother from Dr. W. Knight, Professor of Natural Philosophy in Marischal College, Aberdeen, dated June 8, 1833.

“ I regret that I have not written sooner in reply to your letter of the 9th ult., but I waited until I should find a convenient opportunity for repeating many of the experiments which I hinted the general nature of to you in my letter of the 19th of April. These opportunities have unfortunately not occurred yet, from myself being engaged with a daily class here, and my family at a country residence at some distance; nor are they likely to occur soon, so that I must content myself with

* The sounds from the statue of Memnon and the mountain at Carnac, and those from machinery and a fire-grate, have already been explained, nearly in the above words, by Sir John Herschel, (see Lond. and Edinb. Phil. Mag. vol. i. p. 221,) but without reference, however, to the new phenomena of vibrating metals, with the cause of which, indeed, contrary to Mr. Trevelyan's opinion, we apprehend that they are not essentially connected. The sounds heard on the granite rocks of the Orinoco are attributed by Sir J. Herschel, in the same letter, to sonorous vibrations of the air passing through small orifices, either subterranean or communicating with the atmosphere. The singing in a tea-kettle, and the note given out by stills, are referrible, we conceive, to another order of causes, connected with the rapid condensation of vapour, and quite distinct from all the former as well as from those concerned in the vibration of heated metals.—Ed.

sending you a shorter account than I could wish of the new trials made on the vibrations of metallic bodies in the end of March and beginning of April last.

“ Having frequently failed as well as succeeded with the iron pokers and masses of lead, &c., and not being always able to account for the failures, a simple plan of producing vibrations occurred to me, which in no instance yet fails upon the greater number of metals employed. This consists in placing a quantity of a melted metal (as tin, lead, fusible metal, bismuth, &c.) in a hemispherical, or, better, in a parabolic conoidal cup of copper, or iron, or brass, lying above a piece of lead or other metal. The oscillations of the cup thus resting upon a small surface continue long after it has become solid, indeed until the cup and its contents have fallen to a temperature not much above that of the metal upon which they rest. I have employed in many trials cups of iron, brass and copper, of various sizes, from 2 to 6 inches in diameter at the top; and of melted metals, from an ounce to some pounds, of bismuth, tin, lead, zinc, &c. (In case of pouring the melted metal too rapidly from the ladle into the cups, and thus overturning the latter, it may be steadied by a small pair of forceps for a few seconds.) No vibrations could be produced *above* any other bodies than metals: they are most striking *above* lead, tin, zinc, antimony; more feeble above silver, gold, platina, brass, copper. Above wrought and cast-iron, I could not observe them. They are very distinct above slabs of the metal of reflecting telescopes, and above fusible metal. The smoothness of the ingots above which the vibrations are performed lessens the effect greatly, or prevents it.

“ Among the most interesting trials are:

“ 1st, 3 or 4 ounces of lead, melted and poured into the parabolic cup of copper, resting above the ingot of 1 pound of tin with uneven surface. The tremors loud, quick and large; *a peculiar crackling noise* is heard, which I cannot distinguish in character from the noise of the same kind which ensues on bending a piece of block tin held close to the ear. On compressing the tin ingot with the fingers, the sound does not cease, but assumes a sort of stifled character: on removing the fingers, the former crackling noise is resumed. The fingers feel distinctly the vibration communicated to the ingot by the heated cup. •

“ 2nd, The same placed on an ingot of 1 pound of zinc. The vibrations commenced immediately on pouring out the lead: they were small, rapid and equal, (not of an unequal sort like those above antimony): the sounds louder than above any other metal. They cease on pressing the ingot between the fingers; when resumed, there seemed some difference of tone: the stopping at the end sudden and distinct.

“ 3rd, The vibrations of the cups above a platina coin were very slow, and did not commence until after the lead had become solid: they soon ceased;—as if connected with the bad conducting power of platina?

“ 4th, The solidifying of the melted metals in the cup, and their crystallization when made to solidify during their vibrations, offered several singular phænomena, particularly with bismuth, lead and tin. As far as trials have yet gone, the crystallizing property seemed to be more conspicuous when solidifying during the vibrations, than when the same quantities of metals were not vibrating.

“ These observations, like those of your brother, seem to connect this subject closely with the arcana of cohesion. On seeing the copy of your brother’s paper, which you were so good as to send me, on the 19th of April, I was naturally much interested in the perusal, and cannot but think his theory the true one. An extensive field seems to be opened for examining the passage of heat through metallic bodies, in connexion with the structure of their atoms in cohesion. I could wish much that he or you would repeat some of the above experiments. I do not know when I shall find time to resume them. I found indeed, soon after beginning, that I could not advance without one of the small metallic Breguet’s thermometers, with which the temperature of the ingots might be approximated to. I omitted to mention that the cups themselves will vibrate, if heated above a gas flame; but the other mode of pouring from a ladle some melted metal is more convenient, and increases the range of the phænomena; or hot mercury may be poured in, but the fumes are disagreeable.

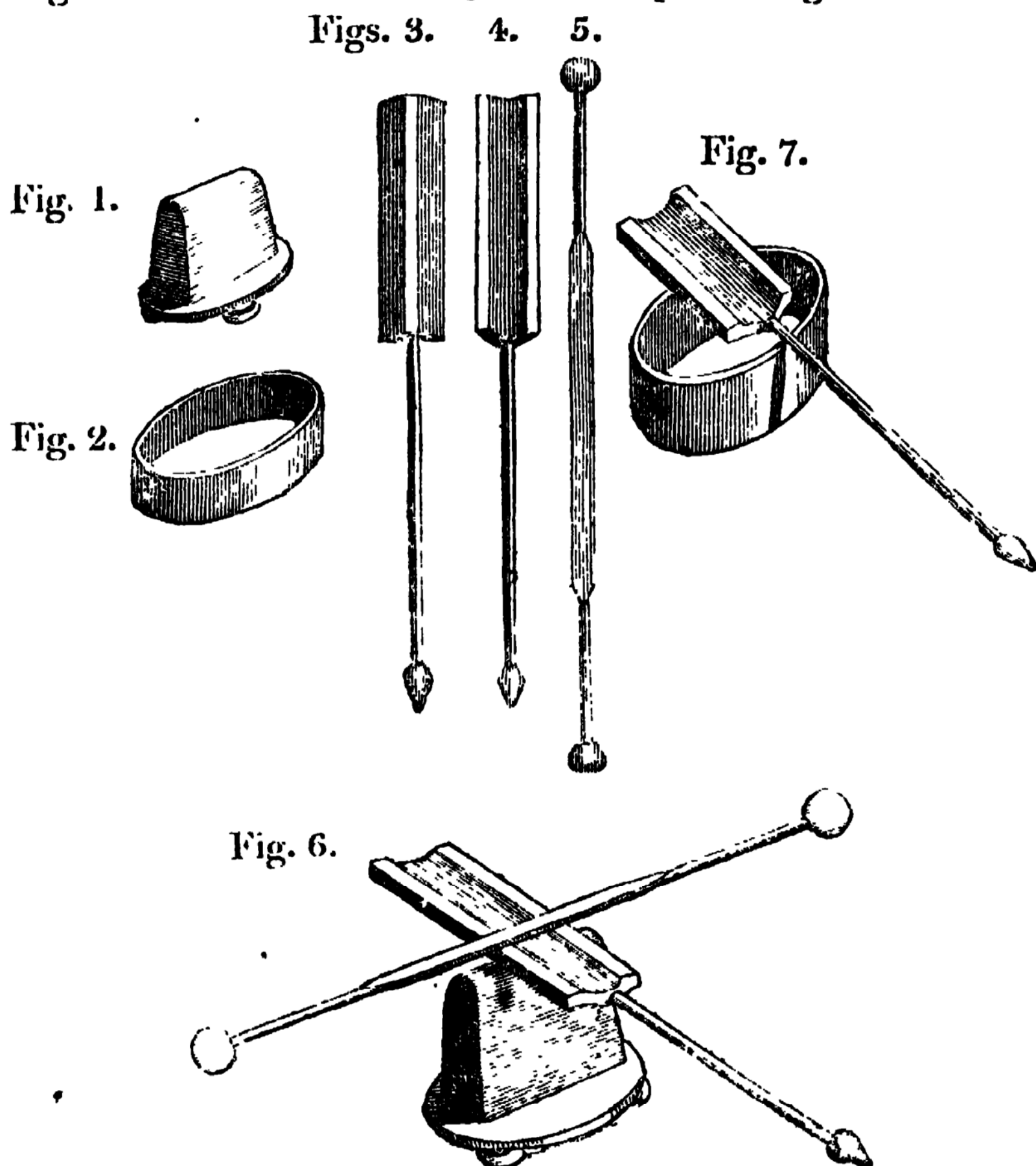
“ P.S. If you try any of the experiments, it will give me great pleasure to hear of your success. I have tried them so very often that I am quite sure you will not lose your trouble.”

At one of the meetings of the Royal Society of Edinburgh this spring, Professor Forbes, of the Edinburgh University, read an able paper, and one that showed great research, on the vibration of metals; he also produced tables showing the difference of metals in the conduction of heat and electricity, and for showing the vibrations: there was great similarity in the position of the different metals in the three columns of the table, showing the difference of power in producing the three various effects*.—In the Literary Gazette for May 17, 1831, Mr. Faraday is mentioned to have shown the experiment of a curved silver plate vibrating and sounding when placed on cold iron, and to have stated that this effect had long been known to working silversmiths.

* An abstract of Prof. Forbes’s paper will be found in our last Number, p. 303.—EDIT.

Description of the Figures.

- Fig. 1. Is the lead block on the rounded surface of which the bar rests, by that means the vibrations being more distinctly seen: this block is screwed to a flat brass plate, having three small flat knobs to rest on.
- Fig. 2. Shows the ring on which the bar is to be placed to produce the tone: it is better to have an unequal notch for the bar to rest on, thus 
- Fig. 3. Is the back part of the bar, showing the ridge on which it ought to rest when placed on the lead.
- Fig. 4. Is the upper side of the bar, hollowed out in the centre so as to bring the gravity more to each side.
- Fig. 5. Is the bar with a ball at each end, to be laid across the vibrating bar, so as to cause it to move through a larger arc.
- Fig. 6. Shows how the bars ought to be arranged for exhibiting both the vertical and lateral motion.
- Fig. 7. Shows the bar as placed for producing the tone.



same glass be placed as at $lmni$, and inclined to the direction of the incident light, then very frequently (that is, when the apparatus used is properly adjusted and applied,) there is another set of bands seen at f . In this case it will be seen that the upper ray aqr , will have passed through a greater distance in air than the lower ray bst . But on account of the glass $lmni$ being more inclined to the ray passing through it than the glass $ghik$ to the ray aqr , it will be seen that the lower ray will have passed through a greater distance in glass. Or the sum of the distances aq and rf is greater than the sum bs and tf , and the distance in glass st is greater than qr .

Now, putting $d =$ the distances in air $aq + rf$
 and $d_1 = \frac{bs + tf}{}$
 and putting $D =$ the distance in glass qr
 $D_1 = \frac{st}{}$

Then, when the interference at the point f is produced between rays which have set out *simultaneously* from a and b , putting $T =$ the whole time of passing from a or b to f , and
 $v =$ the velocity of light in air
 $w = \frac{}{}$ in glass,

we have $T = \frac{d}{v} + \frac{D}{w} = \frac{d_1}{v} + \frac{D_1}{w}$

from which we find $\frac{v}{w} = \frac{d - d_1}{D_1 - D}$

When, therefore, we can find by experiments the distances in air and glass, we can also find the relative velocities of light in them. This, however, it must be remembered, is on the supposition that the velocity is the same in glass for all incidences, and that the velocity in air is the same after having passed through the glass as before. The former of these points we shall have to refer to again presently.

In the apparatus I used for the experiments about to be described, the luminous points a and b were produced by reflections of the image of the sun given by an equi-convex lens of crown glass of $\frac{1}{10}$ th inch focus, from two mirrors of speculum metal. These mirrors were set in a frame, and so that their contiguous edges could be adjusted by means of four fine-threaded screws. The glasses $ghik$, $lmni$, were produced by dividing a piece from one plate, which had been examined very carefully to see that the surfaces were flat and truly parallel. The mode of examining the parallelism of the surfaces which I adopted, was that of viewing the reflected images they gave of a narrow line of light in the following manner.—Standing in a room facing the window through which

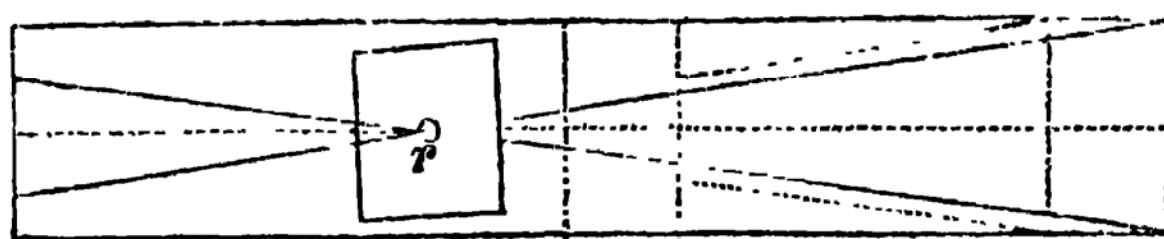
the light from the sky came directly, and at the distance of a few yards from this window, I held up before me the plate (a small one) with a piece of black velvet behind it; then holding a piece of fine platinum wire (a very fine steel needle would answer as well,) directly before the pupil of one of my eyes, I viewed with the same eye the images, produced by the surfaces of the glass, of the fine line of light which was shown by the platinum wire. When the surfaces are even very slightly inclined to each other, it is immediately detected by their two images being distinctly separated; but when both surfaces are accurately perpendicular to the same incident rays, the two reflected images become blended into one. Knowing it to be a very difficult task to produce two such flat and parallel surfaces on a small scale, I did not attempt to grind and polish a plate myself, but chose rather to avail myself of the kindness of a friend, to select a small plate, to suit my purpose; and I considered myself fortunate in finding *one* amongst a large number of plates in which no inclination of the surfaces could be detected in any direction. One of the best methods for examining the truth of flat surfaces, I find to be that of pressing together alternately the surfaces of the two pieces of glass, when, if there be any appreciable curvature, it will be detected by an appearance of Newton's rings; care must, however, be had that the curvature does not arise from a too great degree of pressure.

My two pieces of glass from the plate just mentioned were attached, the one to the pivot of a divided circle, and the other to the arm turning on this pivot, which carried the reading microscope. The circle was something less than 18 inches in diameter; and from the examination I made of the values of the divisions, I think the angles, in the experiments which were deduced from the divisions for even tens of degrees, could not be wrong more than five or six seconds. For the other divisions it is possible the error might be several times as much, but nothing, as I have proved, which could affect fundamentally the results. The heliostat I used was the one I have described in a former number of the Magazine, (Lond. and Edinb. Phil. Mag. and Journal, vol. ii. p. 6,) and the lens and two mirrors were attached to it, so that the line cp , fig. 1, might be horizontal. The plane of the circle was adjusted accurately to be parallel to this line, and perpendicular to the horizon, by three screws forming the feet of its support. The two glasses being cemented to plates of brass were also adjusted by three screws each, so as to be perpendicular to the plane of the circle. The distance ef in the figure was measured by means of a chased steel screw set accurately in a brass frame; this screw was set perpendicular to the horizon,

and it carried up or down, when turned, a piece of hard wood, to which was attached the eye-lens with a needle-point in its focus. By means of an index and a divided card, the motion of the screw was read off to the 200th part of a revolution.

The distance of the points a and b was determined by the method used in a similar case by M. Fresnel, namely, by placing a piece of tin-plate, so that a circular perforation in it might be on the line ep , as in fig. 2. A series of coloured rings round a centre, also coloured, but well defined, was in

Fig. 2.



this manner seen for each luminous point a and b , and the distance ii of these centres was very easily and accurately determined with the screw carrying the eye-glass; from which the distance ab was easily deduced, when the distances er and rp had been measured.

There are many points that require great attention in conducting the experiment, with which I must not trespass longer on the reader's patience, but will merely state that they arise from the necessary smallness of the distance ab , the diffracted fringes of the edges of the glasses hi , li , the peculiar adjustments required for the mirrors, the positions of the mirrors with respect to the original luminous point, and the positions of the glasses with respect to the points e, f and a, b .

The *final* adjusting of the apparatus often required all the sun-light I could obtain, for one or two days before the experiment could be proceeded with; and this occurring every time the apparatus was altered so as to experiment under different circumstances, prevented me, even during the long succession of fine weather we had this spring, from obtaining more numerous measures. The alterations of the apparatus were to produce different values for the distance ab , and the proper places of the edges hi , li , for the different angles between the glasses. For all the measures in which the glasses were inclined to each other *more* than 10 degrees, the two sets of fringes at e and f were seen, and afforded the means of taking the distances of these points with great accuracy. In the others, one set only could be seen at once, so that the lower glass had to be brought up to a parallel position with the upper one for every measure; the lower pencil passing in

this case through the lower glass, for both sets of fringes. The light I used was a red obtained by extracting the colouring matter of alkanet root in oil of turpentine, and then transferring it, when filtered, by solution and quick evaporation, to Canada balsam. This coloured balsam, placed between two plane glasses and cemented round with sealing-wax, furnished a convenient way of getting light of any required purity: that which I used was such as to cause rather more than twenty bright and twenty dark bands to be visible at once; and yet in this number there was no difficulty, with a little practice, in determining with certainty which was the central one.

The following are the measures I have taken, and the results which they give.

On March 30th 1833, the angle between the glasses ($\zeta i \gamma$) being $14^{\circ} 1' 7''$,

the distance ef was found to be $\cdot 29290$ inch.

When the angle ($\zeta i \gamma$) was $15^{\circ} 0' 15''$,

ef was found to be $\cdot 34320$.

For both the above ii , fig. 2. = $\cdot 10126$

er — = $12\cdot 15$ inches

rp — = $19\cdot 65$ inches

whence $ab = \cdot 1637661$.

The thickness of the glass plates was found to be $\cdot 1375$ inch. Taking the refractive index of the glass (μ) at $1\cdot 495$, these measures give

$$\frac{d-d_1}{D_1-D} = \frac{\cdot 002812115}{\cdot 001743651} = 1\cdot 612.$$

The second one for the angle $15^{\circ} 0' 15''$ I have not calculated.

On the 6th of April, I obtained the following:

Angle $\zeta i \gamma$, fig. 1 = $18^{\circ} 59' 17''$

ef — = $\cdot 540276$

ii fig. 2. = $\cdot 104012$

er — = $12\cdot 15$

rp — = $19\cdot 9375$

whence ab = $\cdot 170678$

from which we find when μ is taken = $1\cdot 495$

$$\frac{d-d_1}{D_1-D} = \frac{\cdot 0051619}{\cdot 0030902} = 1\cdot 6704.$$

On the 21st of April, I got the following measures:

$$\begin{aligned}
 \text{Angle } \zeta i \gamma, \text{ fig. 1.} &= 5^\circ 0' 6'' \\
 ef &= \cdot 0337193 \\
 ii \text{ fig. 2.} &= \cdot 1125248 \\
 er &= 12\cdot 10 \\
 rp &= 20\cdot 04,
 \end{aligned}$$

This value of ii being taken without removing the glasses, on account of the great labour required to re-adjust the apparatus, requires to be considered as for the distance rp corrected by aa , fig. 2, which I calculated in the experiments taken at and after this date from the expression:

$$aa = \text{thickness of glass} \times \left(\frac{\mu - 1}{\mu} \right)$$

whence $ab = \cdot 1859400$ and for $\mu = 1\cdot 495$

$$\frac{d - d_1}{D_1 - D} = \frac{\cdot 00037980}{\cdot 00024464} = 1\cdot 552.$$

On the same day, I obtained the following also:

$$\begin{aligned}
 \text{Angle } \zeta i \gamma, \text{ fig. 1.} &= 9^\circ 59' 42'' \\
 ef &= \cdot 1240136 \\
 ii \text{ fig. 2.} &= \cdot 11339 \\
 er &= 12\cdot 10 \\
 rp &= 20\cdot 04
 \end{aligned}$$

whence $ab = \cdot 1873696$, and when $\mu = 1\cdot 495$

$$\frac{d - d_1}{D_1 - D} = \frac{\cdot 001419883}{\cdot 00092721} = 1\cdot 531.$$

On the 8th of May, I obtained as follows:

$$\begin{aligned}
 \text{Angle } \zeta i \gamma, \text{ fig. 1.} &= 5^\circ 0' 6''. \\
 ef &= \cdot 047953687 \\
 ii \text{ fig. 2.} &= \cdot 0776168 \\
 er &= 12\cdot 05 \\
 rp &= 20\cdot 28
 \end{aligned}$$

whence $ab = \cdot 1303349$, and taking $\mu = 1\cdot 405$,

$$\frac{d - d_1}{D_1 - D} = \frac{\cdot 00037257}{\cdot 00023757} = 1\cdot 568.$$

On May 9th, I obtained the following:

$$\begin{aligned}
 \text{Angle } \zeta i \gamma, \text{ fig. 1.} &= 5^\circ 0' 6'' \\
 ef &= \cdot 04641225 \\
 ii \text{ fig. 2.} &= \cdot 0803153 \\
 er &= 12\cdot 05 \\
 rp &= 20\cdot 26
 \end{aligned}$$

whence $ab = \cdot 1347321$, and taking here $\mu = 1\cdot 492$

$$\frac{d - d_1}{D_1 - D} = \frac{\cdot 00037475}{\cdot 00023915} = 1\cdot 567.$$

On May 18th, I obtained the following :

Angle	$\zeta i \gamma$, fig. 1.	$= 17^\circ 0' 55''$
	ef —	$= \cdot 4131553$
	ii fig. 2.	$= \cdot 11176437$
	er —	$= 12\cdot 08$
	rp —	$= 20\cdot 23$

Another set,

Angle	$\zeta i \gamma$, fig. 1.	$= 16^\circ 0' 23''$
	ef —	$= \cdot 44349222$
	ii fig. 2.	$= \cdot 09132167$
	er —	$= 12\cdot 08$
	rp —	$= 20\cdot 23$

On re-examining the adjustments, after taking these two sets, I found the glasses had got slightly moved. It is probable that this had occurred in taking the measures of er and rp , and therefore would not influence the others ; but I have not calculated them.

On May 22nd, I obtained the following :

Angle	$\zeta i \gamma$, fig. 1.	$= 19^\circ 59' 39''$
	ef —	$= \cdot 70650$
	ii fig. 2.	$= \cdot 0897189$
	er —	$= 12\cdot 08$
	rp —	$= 20\cdot 25$

whence $ab = \cdot 1500612$; and when $\mu = 1\cdot 492$,

$$\frac{d-d_1}{D_1-D} = \frac{\cdot 00576170}{\cdot 00334260} = 1\cdot 7237.$$

On May 23rd, I obtained the following :

Angle	$\zeta i \gamma$, fig. 1.	$= 19^\circ 59' 39''$
	ef —	$= \cdot 6719927$
	ii fig. 2.	$= \cdot 09218843$
	er —	$= 12\cdot 08$
	rp —	$= 20\cdot 29$

whence $ab = \cdot 1544955$, and

$$\frac{d-d_1}{D_1-D} = \frac{\cdot 00568722}{\cdot 00335378} = 1\cdot 6958.$$

when the refractive index μ is taken $= 1\cdot 495$.

When we take $\mu = 1\cdot 492$, we find

$$\frac{d-d_1}{D_1-D} = \frac{\cdot 00570817}{\cdot 00336775} = 1\cdot 6949.$$

The reason of my having used two values of the refractive index is this. The circle being engaged with the other part of the experiments, and as I possessed no other divided instrument

fit for determining the refractive power of the glass, I had recourse to Dr. Wollaston's instrument, with which I deduced 1.495 as the refractive index. When the circle was at liberty, I found this to be rather too high for the red light with which I had been experimenting, and that 1.492 was about the true refractive index for it. On going, however, through the calculation again for the experiment of the 23rd of May, the difference was so small in the results, that I have not thought it needful to re-calculate the others, and particularly as the labour is considerable.

It will be seen in fig. 1, that to find the true value of the angle qae , from knowing the distances cf , ca and the thickness of the glass $\beta\alpha$, it is necessary to find the distance aa_1 ,

from the formula $aa_1 = t \left(1 - \frac{\cos \phi}{\mu \cos \phi'} \right)$, where $t = \beta\alpha \phi =$

the angle of incidence qae , and $\phi' =$ the angle of refraction. By repeating this process, we at length find the angle qae to any required correctness, and then easily find d and D .

For the lower ray, supposing a perpendicular ($b\gamma$) to be let fall upon the lower glass, σb is first found by the same formula as aa_1 , and then from it $\sigma\tau$ or $b\varrho$, as may be most convenient to use. After one or two approximations in this manner, the values of the angles sbd and sbg may be found with every accuracy that the logarithmic tables to 7 figures admit of. Knowing the angle sbg , we soon find $ts = D_1$, then from it we obtain the value of $\theta\eta$, and finally, $d_1 = sb + ft = (db - \theta\eta) \times \secant sbd$.

In the calculations, I have used the logarithmic tables to 7 figures, excepting for the secants of the angles qae and sbd , in which, from the largeness of the values of d and d_1 , compared with their differences, I found it necessary to calculate these secants from the formula, $\secant = \sqrt{1 + \tan^2}$, to 10 places of decimals.

According to the undulatory hypothesis, we should have for all incidences

$$w : v :: (D - D_1) : (d - d_1) :: 1 : \mu$$

$$\text{or, } \frac{v}{w} = \frac{d - d_1}{D - D_1} = \mu.$$

We see that this is not supported by the preceding experiments, and that they deviate the more from it as the angle of incidence on the lower glass is greater. This leads us at first to suppose the velocity of light in glass to be variable for different incidences. Before concluding finally on our results, we shall find it wise on this, as on many other occasions, to doubt

the infallibility of the undulationists. They tell us, it is true, that the image of the sun's disk, produced by a lens of short focal length, may be used instead of a luminous body, in *all* experiments in which it is wished to produce a series of waves diverging from a point. And the measurements hitherto taken, which have been supposed by some to demonstrate the undulatory theory, have been made with common lenses, without its being imagined there was any need of achromatic, apla-natic glasses for these delicate purposes.

Without attempting to clear the foregoing results of the effects of aberration, we may still convince ourselves by a simpler procedure, that their discordance with the hypothesis,—that the velocity of light in passing through transparent bodies is inversely as their refractive indices,—is more ap-parent than real.

The experiments appear to show that the velocity (w) of the light in the glass was slower as the incidence was in-creased, making the fraction $\frac{v}{w}$ of larger value. Then the light must also have moved more slowly in the lower glass than in the upper one, on account of its greater incidence upon it.

Let us, therefore, instead of

$$T = \frac{d}{v} + \frac{D}{w} = \frac{d_1}{v} + \frac{D_1}{w}$$

take the w in the denominator on the second side equal to $z w$, and we have

$$T = \frac{d}{v} + \frac{D}{w} = \frac{d_1}{v} + \frac{D_1}{z w}$$

$$\text{and} \quad z = \frac{\frac{v}{w} D_1}{(d - d_1) + \frac{v}{w} D}$$

In applying this to the experiments of the 9th of May, let us assume, as the experiments warrant us in doing, that the velocity in the glass at a perpendicular incidence is to the ve-locity in air in the inverse ratio of their refractive indices. We find for z the following :

$$z = .999912$$

$$\text{and from this} \quad \frac{v}{z w} = 1.49213$$

which is nearly, as we supposed it, at a perpendicular inci-dence, namely, 1.492.

For another example, the experiment of May 23rd gives

$$z = \cdot 996759$$

and $\frac{v}{z w} = 1\cdot 49685.$

Or, the ratio of the velocities appears still to be, when fully considered, nearly 1·492, as we have taken it to be, at an incidence nearly perpendicular.

These experiments, then, give no adequate ground for maintaining the incorrectness of the hypothesis that the velocity in refracting bodies is inversely as their refractive indices.

On a careful re-examination of the thickness of the glass, of the value of the threads of the screw by which were measured the distances *iz* and *ef*, and of the other parts of the apparatus, I could find no ground to suppose the results I had obtained were influenced materially by any other imperfection besides the aberration at the focus of the lens.

I shall consider it exceedingly important to determine whether, when an achromatic lens of short focus is used to form the luminous point, the central band of direct interference given by two mirrors is black, as it has appeared to me, and to several friends to whom I have shown it, when adequately tried with a common lens,—contrary to what the undulationists would have us believe it to be. If the central band shall still be found black, I shall submit to those who are not perfectly wedded to the undulatory theory, that it *must fall*. If, however, when adequately tried, the experiment shall exhibit the central band white, I shall consider *one of my greatest* difficulties to receiving the undulatory theory to be removed.

LVI. *Descriptions of several new British Forms amongst the Parasitic Hymenopterous Insects.* By J. O. WESTWOOD, F.L.S. &c.*

[Continued from vol. ii. p. 445.]

25. *Streblocera*, Westw.

ALYSIÆ affinis. Caput anticè bituberculatum. Antennæ ferè longitudine corporis 16-articulatæ, articulo 1mo longo (capite longiori) subtùs dente valido armato, 2do brevi apice prioris obliquè inserto, 3tio paullò majori, 4to etiam obliquè inserto, hoc et reliquis filiformibus. Collare angustum. Abdomen subrhomboideum pedunculo brevi. Alæ anticæ stigmatè magno, areolâ 1 marginali brevi, areolâ 1mâ submarginali cum discoidali effusâ.—*Strebl. fulviceps*, Westw. Piceo-nigra, nitida, capite fulvo, oculis ocellisque nigris, antennis fuscis, articulis 3 basalibus fulvis, pedibus fuscis, femoribus

* Communicated by the Author.

fulvescentibus ; alarum stigmatè nervisque pallidè fuscis. Long. Corp. lin. 1. Coombe, Aug. 1833.

26. *Basalys*, Westw.

Aneurhynchum (antennis) cum *Spilomicro* (alis) arctè conjungens. Os haud rostratum. Antennæ ♂ 14-articulatæ, articulis 2 et 3 brevibus, 4to magno, externè producto, reliquis subæqualibus tenuibus. Alæ magnæ nervis duobus subcostalibus tertiam alæ partem accedentibus, nervo transverso illorum apicem connectente cum nervo ex illorum medio exeunti areolam triangularem formantibus stigmatè magno internè retro-producto, nervisque quibusdam longitudinalibus ferè oblitteratis. — *Bas. fumipennis*, Westw. Niger, nitidus, pilis longis ornatis, alis fuscis, stigmatè nervoque transverso nigris, nervis reliquis fuscis, apice femorum tibiisque piceo, tibiis piceo-nigris. Long. lin. 1½. Prope Londinum. R. Lewis.

27. *Elasmus*, Westw.

Ex *Eulopho typicali* differt antennis ♂, articulis 3, 4 et 5 brevissimis ferè coalitis (singulo ramum longum emittenti), 6to longo, reliquis clavam formantibus; ♀ ut videtur 10-articulatæ, 3 et 4 annuliformibus, 5, 6, 7 oblongis, 8, 9, 10 clavam formantibus, metathorax (♂ ♀) utrinque ad latera in laminam magnam concavam posticè productus. — Alæ angustissimæ. Pedes longissimi femoribus magnis. — *Eulophus flabellatus*. Fonscol. Ann. Sc. Nat., Jul. 1832. (Descr. antennarum vitiosa). Coombe, Aug. 1833.

28. *Stenomesus*, Westw.

Euplectro affinis. Caput lutescens. Antennæ longiores, ♂ graciles, ♀ subincrassatæ, 9-articulatæ, articulo 2do 3tio dimidio breviori, clavâ 3-articulatâ. Thorax anticè collari attenuatus; abdomen pedunculo brevi, ♂ parvum spatuliforme, ♀ majus, ovatum. Alæ magnæ. Pedes sat elongati tibiis intermediis, ♂ attenuatis, apice subclavatis. — 1. *Sten. pulchellus*, Westw. ♂. Capite nigro, antennis nigris, basi subtus pallidis, collari et mesothorace fulvo-testaceis, parapteris nigris metathorace nigro, abdomine nigro, basi maculâ magnâ fulvâ, pedibus fulvis, anticorum femoribus basi tibiisque apice fuscis, intermediarum tibiis apice tarsisque totis nigris; alis immaculatis. Variat collari scutelloque plùs minùsve nigro notatis. Long. lin. ¾. Exp. Alar. 1½. Prope Londinum et Cantabrigiam. — 2. *Sten. maculatus*, Westw. ♀, Capite fulvo maculâ occipitali lineâque pone oculos nigris, antennis nigris articulo basali fulvo, thorace ut in præcedenti colorato, abdomine nigro basi fasciâ tenui lineâque brevi fulvis, pedibus fulvis, tarsorum apice fusco, alis maculâ centrali fusciscenti. An ♀ præcedentis? Long. lin. 1. Exp. Alar. 1½. Prope Londinum. R. Lewis.

29. *Cheiloneurus*, Westw.

Encyrtum (pedibus) cum *Eupelmus* (alis) conjungens. Caput sat magnum. Mandibulæ 3-dentatæ. Antennæ prope os insertæ, ferè thoracis longitudine, versus apicem subclavatæ, 11-articulatæ, articulo 2do 3tio majori. Thorax oblongo-quadratus, anticè collari attenuato, scutello posticè fasciculato; abdomen ovatum depressum posticè acuminatum. Alæ anticæ nervo stigmatali brevissimo à loco conjunctionis nervi subcostalis cum costâ paullo remoto. Pedes ut in *Encyrtis*. — *Cheil. elegans*, Dalm. (*Encyrtus* et *Eupelmus*). Capite obscurè æneo, oculis antennisque nigris; his basi et apice pallidioribus, thorace cinerascens-æneo, scutello flavo, abdomine nigro, cupreo nitenti, alis fuscis, in medio obscurioribus, basi atque prope et sub stigmata albis, pedibus pallidè testaceis, femoribus tibiisque posticis obscuris, tibiisque 4 posticarum basi albo. Long. Corp. lin. ¾. Expans. Alar. lin. 1½. Richmond Park, Aug. 1833.

30. *Ectroma*, Westw.

Encyrtum (pedibus) cum *Theocolace* (habitu) conjungens. Caput crassum mandibulis 3-dentatis. Antennæ inter partem inferiorem oculorum insertæ, ferè corporis longitudine, sensim incrassatæ, 9-articulatæ, articulo 3tio 2do minori, 9no 8vo haud majori. Thorax oblongo-quadratus collari acuminato. Alæ rudimentales.—*Ectr. fulvescens*, Westw. Capite thoraceque pallidè fulvis, æneo submicantibus, abdomine obscurè testaceo metallico, oculis et antennis nigris, his basi subtùs, apiceque articuli terminali pallidis pedibus fulvis, tarsorum apice nigro. Long. lin. $\frac{2}{3}$. Coombe, Aug. 1833.

31. *Pteroptrix*, Westw.

Agonioneuro affinis. Corpus brevissimum, latum, depressum. Caput transversum oculis magnis pilosis. Antennæ 8-articulatæ, articulis 2 et 3 æqualibus, 4to paullò minori, 5, 6, 7 multò majoribus, 8vo minutissimo. Thorax ferè quadratus. Scutellum magnum. Alæ magnæ, apice longè pilosæ, nervo stigmaticali brevi angulum acutum formante. Abdomen sessile, brevissimum, apice mucronatum. Tarsi 4-articulati.—*Pter. dimidiatus*, Westw. Piceo-niger, scutello albido, antennis flavis, articulo 7mo obscuriori, alarum dimidio basali fusco, pedibus pallidis, femoribus tibiisque basi fuscis. Long. Corp. lin. $\frac{1}{4}$. In quercu, Richmond Park, Aug. 1833.

32. *Coccophagus*, Westw.

Agonioneuro affinis, differt antennis 8-articulatis, articulo 2do 3tio minori, hoc et duobus sequentibus ferè æqualibus, ultimis 3iis clavam, vix articulo præcedenti crassiorem, formantibus. Tarsi 5-articulati; nervus stigmaticalis brevis apice conoideus. Habitat in Coccis.—Sp. 1. *Entedon scutellaris*, Dalm. Sw. Tr. 1825. 365*.—Sp. 2. *Cocc. pulchellus*, Westw. Pallidè flavus, oculis, thorace anticè abdomineque nigris, antennarum flagello thoracisque lateribus obscurioribus, pedibus flavis, tarsorum apice fusco. Long. lin. $\frac{1}{2}$. Exclusus e *Cocc. aceris*, Jun. 1833.—Sp. 3. *Cocc. obscurus*, Westw. Niger, abdomine nitido, antennis fuscescentibus, pedibus sordidè albidis, tarsorum apice fusco, femoribus intermediis basi, posticis totis, tibiisque posticis basi fuscis. Alarum nervo fusco. Long. lin. $\frac{2}{3}$. Habitat cum præcedenti. Sp. 4. *Entedon insidiator*, Dalm. l. c. 371.

LVII. *Characters of some undescribed Genera and Species of*
Araneidæ. By JOHN BLACKWALL, Esq. F.L.S. &c.†

Tribe, INEQUITEIÆ, Latreille.